





AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT
OF THE
Present Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Considerable Parts of the WORLD.

VOL. LXIII. PART I.

L O N D O N :
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M.DCC.LXXIII.

A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it had been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the

reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

At a C O U N C I L, January 28, 1773.

Resolved, That after Volume LXII. the *Philosophical Transactions* be published twice in a year; the first publication to be of the months of November and December of the preceding year, and January and February of the current year, as soon as may be after February, under the name of the "first part" of the volume: and the second publication to be of the remaining months unto the recess of the Society, as soon as may be after the recess, under the name of the "second part" of the volume.

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PHILOSOPHICAL
TRANSACTIONS.

- I. *An Account of the Discovery of the Manner of making Isinglass in Russia; with a particular Description of its Manufacture in England, from the Produce of British Fisheries. In a Letter from Humphrey Jackson, Esq; F. R. S. to William Watson, M. D. F. R. S.*

S I R,

Read Nov. 19, 1772. **Y**OUR distinguished zeal to promote science in general, and particularly such arts as tend to improve the commerce
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of your country, incites me to address to you the following account of the genuine manufacture of foreign isinglass, with the method of making it in England from British materials. If you should judge the subject deserving attention, you will do me great honour in presenting this paper to the Royal Society: but this I beg leave to submit to your judgment and candour, and to subscribe myself,

With great esteem,

S I R,

Your most obliged,

and most obedient

humble servant,

Great Tower-Hill,
June 15, 1772.

Humphrey Jackson.

ALL authors, who have hitherto delivered processes for making ichthyocolla, fish-glue or isinglass, have greatly mistaken both its constituent matter and preparation.

To prove this assertion, it may not be improper to recite what Pomet says upon the subject, as he appears to be the principal author whom the rest have copied *. After describing the fish, and referring to a cut engraved from an original in his custody, he says: ‘ As to the manner of making the isinglass, the finewy parts of the fish are boiled in water, till all of them be dissolved that will dissolve; then the glucy liquor is strained, and set to cool. Being cold, the fat is carefully taken off, and the liquor itself boiled to a just consistency, then cut to pieces, and made into a twist, bent in form of a crescent, as commonly sold, then hung upon a string, and carefully dried.’

From this account, it might be rationally concluded that every species of fish which contained gelatinous principles would yield isinglass: and this parity of reasoning seems to have given rise to the hasty conclusions of those, who strenuously vouch for the extraction of isinglass from sturgeon; but as that fish is easily procureable, the negligence of ascertaining the fact by experiment seems inexcusable.

Every traveller, as well as author, who mentions isinglass, observes that it is made from certain fish

* See Pomet's History of Drugs, and Caspar Neuman's Chemistry, English translations. Hist. Materiæ Medicæ, Vogel. Lewis's Materia Medica. Doë's Institutes of Chemistry.

found in the Danube and rivers of Muscovy. Willughby and others inform us, that it is made of the sound of the * Beluga; Caspar Newman that it is made of the *Huso Germanorum* and other fish, which he has seen frequently sold in the public markets of Vienna. These circumstances make it appear the more extraordinary, that a perfect account of the manufacture of such an essential article of commerce should remain so long unrevealed.

In my first attempts to discover the constituent parts and manufacture of isinglass, relying too much upon the authority of some chemical authors, whose veracity I had experienced in many other instances, I found myself constantly disappointed. Glue, not isinglass, was the result of every process; and although, in the same view, a journey to Russia proved fruitless, yet a steady perseverance in the research proved not only successful as to this object, but, in the pursuit to discover a resinous matter plentifully procureable in the † British fisheries, which has been found, by ample experience, to answer similar purposes. It is now no longer a secret that our ‡ lakes and rivers in North America are stocked

* Vide Specimen Histor. Nat. Volg. Auctore J. R. Forster, Philos. Transf. 1767.

† Upwards of forty tons of British isinglass have been manufactured and consumed since this discovery was first made.

‡ As the lakes of North America lie nearly in the same latitude with the Caspian Sea, particularly Lake Superior, which is said to be of greater extent, it was conjectured they might abound with the same sorts of fish, and, in consequence of public advertisements distributed in various parts of North America, offering premiums for the sounds of sturgeon, and other fish, for the purpose of making isinglass, several specimens of fine
with

with immense quantities of fish, said to be the same species with those in Muscovy, and yielding the finest isinglass, the fisheries whereof, under due encouragement, would doubtless supply all Europe with this valuable article.

But to return, no artificial heat is necessary to the production of isinglass, neither is the matter dissolved for this purpose; for, as the continuity of its fibres would be destroyed by solution, the mass would become brittle in drying, and snap short asunder, which is always the case with glue, but never with isinglass. The latter, indeed, may be resolved into glue with boiling water, but its fibrous recomposition would be found impracticable afterwards, and a fibrous texture is one of the most distinguishing characteristics of genuine isinglass. The reproduction of leather might, with equal reason, be attempted from the former.

A due consideration that an imperfect solution of isinglass, called fining by the brewers, possessed a peculiar property of clarifying malt liquors, induced me to attempt its analysis in cold subacid menstruums. One ounce and a half of good isinglass, steeped a few days in one gallon of stale beer, was converted into good fining, of a remarkable thick consistence: the same quantity of glue, under similar treatment, yielded only a mucilaginous liquor, resembling diluted gum-water, which, instead of clarifying beer, increased both its tenacity and turbidness, and communicated other properties in no

isinglass, the produce of fish taken in these parts, have been lately sent to England, with proper attestations as to the unlimited quantity which may be procured.

respect

respect corresponding with those of genuine fining. On commixing three spoonfuls with a gallon of malt liquor, in a tall cylindrical glass, a vast number of curdly masses became presently formed, by the reciprocal attraction of the particles of isinglass and the feculencies of the beer, which, increasing in magnitude and specific gravity, arranged themselves accordingly, and fell in a combined state to the bottom, through the well-known laws of gravitation; for, in this case, there is no elective attraction, as some have imagined, which bears the least affinity with what frequently occurs in chemical decompositions.

These phenomena are adduced here as correlative proofs of the impracticability of making isinglass by the previous reduction of the finewy parts of fish into jelly; and it seems evident, that the clarifying action of isinglass depends principally upon a crude minute division, not solution of its parts, which is still farther confirmed, by diluting a few drops of fining with fair water in a glass; for thus the slender filaments become conspicuous to the eye, especially when assisted with a double convex lens, but these immediately disappear on an addition of hot water.

As the general processes for making isinglass appear from hence illusive and erroneous, the long concealed principles of its manufacture into the various common forms and shapes become more obvious and comprehensive. If what is commercially termed long or short stapled isinglass be steeped a few hours in fair cold water, the entwisted membranes will expand, and reassume their original beau-

ful * hue, and, by a dextrous address, may be perfectly unfolded. By this simple operation, we find that isinglass is actually nothing more than certain membranous parts of fishes, divested of their native mucosity, rolled and twisted into the forms above-mentioned, and dried in the open air.

The sounds, or air-bladders of fresh-water fish, in general, are preferred for this purpose, as being the most transparent, flexible, delicate substances. These constitute the finest sorts of isinglass; those called book and ordinary staple, are made of the intestines, and probably the peritonæum, of the fish. The Beluga yields the greatest quantity, as being the largest and most plentiful fish in the Muscovy rivers; but the sounds of all fresh-water fish yield, more or less, fine isinglass, particularly the smaller sorts, found in prodigious quantities in the Caspian sea, and several hundred miles beyond Astracan, in the Wolga, Yaik, Don, and even as far as Siberia, where it is called *kle* or *kla* by the natives, which implies a glutinous matter; it is the basis of the Russian glue, which is preferred to all other kinds for its strength.

The anatomy and † uses of the sound in fish seems not yet adjusted by ichthyologists. I have not met with a genuine description of its situation and

* If the fine transparent isinglass be held in certain positions to the light, it frequently exhibits beautiful prismatic colours.

† Fishermen have a dextrous art in perforating the sound of fresh-taken cod fish with a needle, in order to disengage the inclosed air. Without this operation, the fish could not be kept under water in the well-boat, consequently could not live; but if by accident the operator wounds an artery, the fish presently dies, through the discharge of blood, to the loss of the proprietor, who thus can seldom bring it sweet to market.

figure in any author. A modern writer * will have it to be the mesentery of the fish; but the celebrated Gouan, the latest, and perhaps the most accurate author on ichthyology, gives a more satisfactory and comprehensive account of it, under the title of † *La Vésicule Aérienne*. Yet, if the identity of the air-bladder, and what, in English, is called sound, be admitted, which seems particularly ascertained in a certain genus, viz. the *Afellus* of Willughby, or *Gadus* of Artedi, his description is a little erroneous with respect to its termination near the *Vesica urinaria*; for in cod and ling, the continuation of the sound, or air-bladder, may be easily traced from thence to the last *vertebra* adjoining the tail.

The sounds, which yield the finer isinglass, consist of parallel fibres, and are easily rent longitudinally; but the ordinary sorts are found composed of double membranes, whose fibres cross each other obliquely, resembling the coats of a bladder; hence the former are more readily pervaded and divided with subacid

* Doffie, in *Memoirs of Agriculture*.

† *La Vésicule aérienne* est un sac membraneux composé de deux ou trois envelopes, qui se séparent facilement, & rempli d'air, à la faveur duquel les poissons se soutiennent dans l'eau. Il est pour l'ordinaire situé en long, enfermé dans le peritoine, placé entre les vertèbres & l'estomac. Sa longueur dépend de la capacité du bas ventre, & de la grandeur du poisson: il est tantôt cylindrique, elliptique, ové ou renversé, tantôt à deux lobes & à deux loges, tantôt à trois lobes & à trois loges, &c. dans les males il descend presque jusqu'à la région de la vessie urinaire.

Cette Vésicule est attachée avec l'estomac, avec l'œsophage, sans le diaphragme, tantôt par le côté tantôt par la pointe & s'y abouche par un conduit pneumatique. Gouan, *Histoire des Poissons*.

liquors;

liquors ; but the latter, through a peculiar kind of interwoven texture, are with great difficulty torn asunder, and long resist the power of the same menstruum ; yet, when duly resolved, are found to act with equal energy in clarifying liquors.

Ifinglass receives its different shapes in the following manner.

The parts, of which it is composed, particularly the sounds, are taken from the fish while sweet and fresh, slit open, washed from their slimy *fordes*, divested of every thin membrane which envelopes the sound, and then exposed to stiffen a little in the air. In this state, they are formed into rolls about the thickness of a finger, and in length according to the intended size of the staple : a thin membrane is generally selected for the center of the roll, round which the rest are folded alternately, and about half an inch of each extremity of the roll is turned inwards. The due dimensions being thus obtained, the two ends of what is called short staple are pinned together with a small wooden * peg ; the middle of the roll is then pressed a little downwards, which gives it the resemblance of a heart shape, and thus it is laid on boards, or hung up in the air to dry. The sounds, which compose the long staple, are larger than the former ; but the operator lengthens this sort at pleasure, by interfolding the ends of one or more pieces of the sound with each other. The extremities are fastened with a peg, like the former ; but the middle part of the roll is bent more considerably down-

* See the annexed Drawings [TAB. I. Fig. 1.]

wards; and, in order to preserve the * shape of the three obtuse angles thus formed, a piece of round stick, about a quarter of an inch diameter, is fastened in each angle with small wooden pegs, in the same manner as the ends. In this state, it is permitted to dry long enough to retain its form, when the pegs and sticks are taken out, and the drying completed; lastly, the pieces of isinglass are colligated in rows, by running packthread through the peg-holes, for convenience of package and exportation.

The membranes of the † book sort, being thick and refractory, will not admit a similar formation with the preceding: the pieces therefore, after their sides are folded inwardly, are bent in the center, in such manner that the opposite sides resemble the cover of a book, from whence its name; a peg being run across the middle, fastens the sides together, and thus it is dried like the former. This sort is interleaved, and the pegs run across the ends, the better to prevent its unfolding.

That called cake isinglass is formed of the bits and fragments of the staple sorts, put into a flat metalline pan, with a very little water, and heated just enough to make the parts cohere like a pancake, when it is dried; but frequently it is overheated, and such pieces, as before observed, are useless in the business of fining. Experience has taught the consumers to reject them.

Isinglass is best made in the summer, as frost gives it a disagreeable colour, deprives it of weight, and

* See Fig. 3.

† Fig. 4.

impairs its gelatinous principles; its fashionable forms are unnecessary, and frequently injurious to its native qualities. It is common to find oily putrid matter and *exuviae* of insects between the implicated membranes, which, through the inattention of the cellar-man, often contaminate wines and malt liquors in the act of clarification. These peculiar shapes might, probably, be introduced originally with a view to conceal and disguise the real substance of isinglass, and preserve the monopoly; but, as the mask is now taken off, it cannot be doubted to answer every purpose more effectually in its * native state, without any subsequent manufacture whatever, especially to the principal consumers, who hence will be enabled to procure sufficient supply from the British colonies. Until this laudable end can be fully accomplished, and as a species of isinglass, more easily producible from the marine fisheries, may probably be more immediately encouraged, it may be manufactured as follows.

The sounds of cod and ling bear great analogy with those of the *accipenser* genus of Linnæus and Artedi, and are in general so well known, as to require no particular description. The Newfoundland and Iceland fishermen split open the fish, as soon as taken, and throw the back-bones, with the sounds annexed, in a heap; but, previous to incipient putrefaction, the sounds are cut out, washed from their slimes, and salted for use. In cutting out the sounds, the intercostal parts are left behind, which are much the best; the Iceland fishermen are so sensible of

* See Fig. 5.

this, that they beat the bone upon a block with a thick stick, till the pockets, as they term them, come out easily, and thus preserve the sound entire. If the sounds have been cured with salt, that must be dissolved by steeping them in water, before they are prepared for isinglass; the fresh sound must then be laid upon a block of wood, whose surface is a little-elliptical, to the end of which a small hair brush is nailed, and with a * saw-knife, the membranes on each side of the sound must be scraped off. The knife is rubbed upon the brush occasionally, to clear its teeth; the pockets are cut open with scissors, and perfectly cleansed of the mucous matter with a coarse cloth: the sounds are afterwards washed a few minutes in lime-water, in order to absorb their oily principle, and lastly in clear water. They are then laid upon nets, to dry in the air; but, if intended to resemble foreign isinglass, the sounds of cod will only admit of that called book, but those of ling both shapes. The thicker the sounds are, the better the isinglass, colour excepted; but that is immaterial to the brewer, who is its chief consumer.

This isinglass resolves into fining, like the other sorts, in subacid liquors, as stale beer, cyder, old hock, &c. and in equal quantities produces similar effects upon turbid liquors, except that it falls speedier and closer to the bottom of the vessel, as may be demonstrated in tall cylindrical glasses; but foreign isinglass retains the consistency of fining preferably in warm weather, owing to the greater tenacity of its native mucilage.

* See Fig. 2.

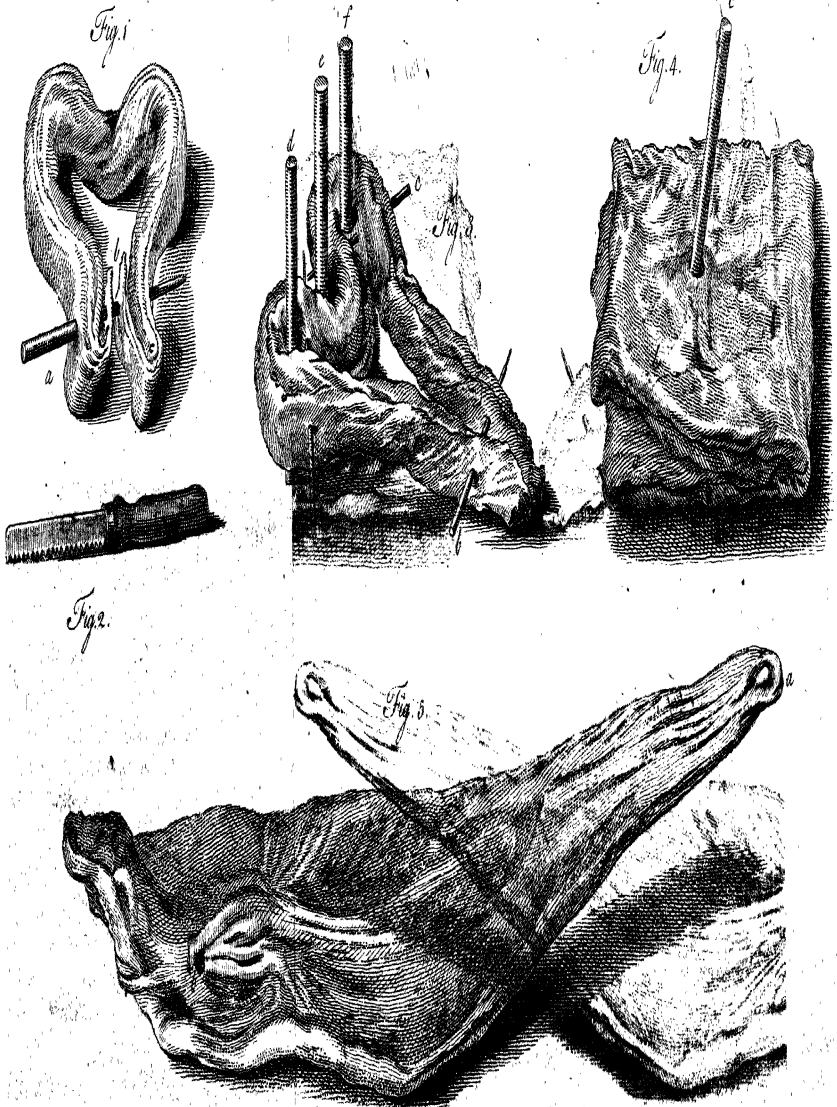
Vegetable acids are, in every respect, best adapted to fining: the mineral acids are too corrosive, and even insalubrious in common beverage.

It is remarkable that, during the conversion of isinglass into fining, the acidity of the menstruum seems greatly diminished, at least to taste, not on account of any alkaline property in the isinglass, probably, but by its enveloping the acid particles. It is likewise reducible into jelly with alkaline liquors, which indeed are solvents of all animal matters; even cold lime-water dissolves it into a pulpy *magma*. Notwithstanding this is inadmissible as fining, on account of the menstruum, it produces an admirable effect in other respects: for, on commixture with compositions of plaster, lime, &c. for ornamenting walls exposed to vicissitudes of weather, it adds firmness and permanency to the cement; and if common brick-mortar be worked up with this jelly, it soon becomes almost as hard as the brick itself: but, for this purpose, it is more commodiously prepared, by dissolving it in cold water, acidulated with vitriolic acid; in which case, the acid quits the jelly, and forms with the lime a *selenitic* mass, while, at the same time, the jelly being deprived, in some measure, of its moisture, through the formation of an indissoluble concrete amongst its parts, soon dries, and hardens into a firm body; whence its superior strength and durability are easily comprehended.

It has long been a prevalent opinion, that sturgeon, on account of its cartilaginous nature, would yield great quantities of isinglass; but, on examination, no part of this fish, except the inner coat of the sound, promised the least success. This being full
of

of *rugæ*, adheres so firmly to the external membrane, which is useless, that the labour of separating them supercedes the advantage. The intestines, however, which in the larger fish extend several yards in length, being cleansed from their mucus, and dried, were found surprizingly strong and elastic, resembling cords made with the intestines of other animals, commonly called cat-gut, and, from some trials, promised superior advantages, when applied to mechanic operations.

Having now sufficiently revealed the principal *ar-
cana* in the manufacture of isinglass, and explained some of its least known phænomena and properties, the farther prosecution thereof, as a commercial business, is left to others, whose future inquiries into the subject, it is hoped, will, in some respect, be anticipated through this narrative; but whatever success may attend the attempt, I flatter myself to stand acquitted, in having contributed every thing in my power to its advancement and perfection.



EXPLANATION of the Figures ; TAB. I.

Fig. 1. Short staple ifinglafs.

a The wooden peg, which fastens the two ends of the ifinglafs.

b The extremities folded inwards.

Fig. 2. The saw-knife.

Fig. 3. Long staple ifinglafs.

a b c Wooden pegs or pins.

d e f Three round sticks, or pieces of wood, fastened in the angles of the ifinglafs by the pins *a* and *c*.

Fig. 4. Book ifinglafs.

a b The two fides, which resemble the cover of a book.

c The wooden pin run through the fides, to fasten them together.

Fig. 5. The entire sound, or *vesica aëria*, of the ifinglafs fish dried in its natural state.

a The hole made for the packthread, where-with it is hung up to dry.

b The orifice of part of the pneumatic vessels left with the sound.

II. *A Letter to Charles Morton, M. D.
Sec. R. S. from Mr. Adam Walker ;
containing an Account of the Cavern of
Dunmore Park, near Kilkenny, in Ire-
land.*

Dublin, April 26, 1771.

S I R,

Read Nov. 19,
1772.

AS I do not find in your Transactions any account of the cave of Dunmore Park, about three miles West of Kilkenny, I beg leave to lay before your learned Society an account of this singular cavern, as near as an eye-survey, and a few experiments on its stones and petrefactions, will admit. It is situated in a fine plain, rising indeed here and there into small hills. The country all round abounds with limestone, and quarries of beautiful black marble, variegated with white shells. Different from those of Derbyshire and Mendip, this cave descends perpendicularly 30 yards, from the top of a small hill, through an opening 40 yards in diameter. The sides of this pit are limestone-rock, whose chinks nourish variety of shrubs and trees, down which the inspector must descend with great caution. In this descent, he is
amused

amused with flights of wild pigeons, and jackdaws from the cave below. When he reaches the bottom, he sees one side of this pit supported by a natural arch of rock, above 25 yards wide, under which he goes horizontally, and sees two subterraneous openings to the right and left. If he turns to the right, he makes his way over rocks and stones, coated with spar in the most whimsical shapes, and formed from the dropping roof, just as the dripping of a candle would cover a pebble. These knobs take a fine polish, are transparent, and variegated with the wildest assemblage of colouring. The Earl of Wandesford had one of them sawn into a slab, and it is as beautiful as a Moco. When I tried these petrefactions with an acid, the effervescence was excessive strong; and, as the earth all round is calcareous, and the stones limestone, I humbly apprehend the icicle figures impending from the roof, and these knobs, are thus formed. The rains, that fall on the hill over this cavern, oozing through an okery calcareous earth, and the limestone roof, imbibe or dissolve their fine particles in their descent; and, as this mixture can only filter through the rock exceedingly slowly, the water hanging on the roof is soon dissolved by the air, and the stony particles are left behind. Hence are formed the icicle-shaped cones that hang from the roof; these growing perpetually longer, have, in many parts of the cave, met the knobs from the bottom, and formed a number of fantastic appearances, like the pillars of a Gothic cathedral, organs, crosses, &c. When the rain filters pretty fast through the roof, it falls on the rocks below, and grows there into knobs and cones,

whose vertex points to those that impend from the roof.

A spectator, viewing these, cannot but conceive himself in the mouth of a huge wild beast, with ten thousand teeth above his head, and as many under his feet. The scene is indeed both pleasing and awful; the candles burning dim, from the moisture in the air, just served to shew a spangled roof perpetually varnished with water, in some places upwards of 20 yards high; in other places we crawled on all-four, through cells that will but admit one at a time. After having scrambled about 500 yards into this (which I will beg leave to call the) right-hand part of the cave, we returned to day-light, and then proceeded to view the left-hand part. Here, as our guides informed us there were many different branches of the cavern, we tied one ball of pack-thread to another, as we went forward, that we might more easily find our way back. This branch is not so horizontal as the other; it inclines downwards, and the openings in it are vastly wider, some being at least 100 yards wide, and above 50 high. A small rill accompanied us, which, by its different falls, formed a sort of rude harmony, well suited to the place. In a standing part of this brook, and near a quarter of a mile from the entrance, we found the bones of a hundred at least of the human race; some were very large, but when taken out of the water, they crumbled away. As we could find nothing like an inscription, or earth for a burying-place, we conjectured that some of the civil wars, perhaps that of 1641, might have driven the owners of these bones into this place. The tradition of the neighbourhood threw no light upon it.

Many

Many of the rocks on the roof and sides of this cavern are black marble, full of white spots of a shell-like figure; and the whole neighbourhood is full of quarries of this beautiful stone, which takes a fine polish, and is used through the three kingdoms for slabs, chimney-pieces, &c. I observed, in some deep and wet parts of these quarries, this elegant fossil in the first stages of its formation; the shells are real, but so softened by time and their moist situation, as to be susceptible of receiving the stony particles into their pores, by whose cohesive quality, they in time become those hard white curls that give value to the marble: and it is very remarkable, and a proof that these white spots have been real shells, and thus formed, that the longer a chimney-piece or slab is used, the more of those spots ripen into view.

I have taken many more notes of the natural curiosities in this kingdom, which I shall be happy to communicate to your respectable Society, if you think the subjects of sufficient importance; and am,

With great respect,

S I R,

Your most obedient,

humble servant,

Adam Walker.

III. *A short Account of some Specimens of native Lead found in a Mine of Monmouthshire: In a Letter from Michael Morris, M. D. F. R. S. to M. Maty, M. D. Sec. R. S.*

Fludyer's-Street, Westminster, Nov. 5, 1772.

S I R,

Read Nov. 19,
1772.

ABOUT the middle of last July, I received three specimens of lead-ore from Valentine Morris, Esq; of Piercefield, in Monmouthshire. They were dug up in one of his fields, on making some drains, at no considerable depth; they were marked N^o 1, 2, 3. On reducing to powder an ounce and a half of the ore, marked N^o 3, in order to assay it, I perceived that several small bits were flatted by the pestle, which, on a farther examination, proved to be native lead. Though the bits of lead are inconsiderable, yet, as they are the first that have been publickly seen in England, or, that I know of, in Europe, some of the best and latest writers on mineralogy declaring that they have not met with any, I thought it my duty to acquaint the Royal Society with the fact, that the first
account

account of native lead may appear in the Philosophical Transactions, as well as the first account of native tin.

N. B. In more than 300 assays of lead ore, I have met with nothing of the kind before..

I am,

Yours. &c.

M. Morris.

IV. *Farther Remarks upon a Denarius of the Veturian Family, with an Etruscan Inscription on the Reverse, formerly considered. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Reverend John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Received June 20, 1772.

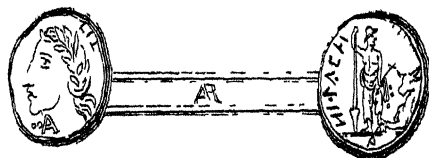
S I R,

I.

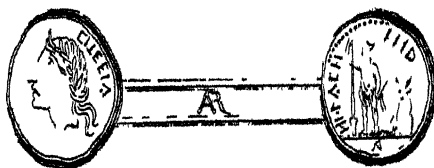
Read Nov. 19, 1772. **S**OME years since (1), I offered my thoughts upon an inedited Samnite denarius, in my small collection, with [See TAB. II. n. 1.] the Samnite-Etruscan letters **VTVJ·II**, as I then apprehended, upon the reverse. But as the two last letters were ill preserved, or rather in part defaced, I was not intirely satisfied with my lection of the

(1) *Philosoph. Transact.* Vol. LVIII. p. 253—261.
inscription

Tab. II. n. 1



Tab. II. n. 2.



inscription to which they appertained. I have, however, since met with the [See T A B. II. n. 2.] same coin, finely preserved, in the very valuable cabinet of the Reverend and learned Dr. Milles (*), Dean of Exeter, President of the Society of Antiquaries, and Fellow of the Royal Society, with three letters, in the room of the two supposititious ones, upon it, perfectly formed; by the assistance of which, I have been enabled to give the true reading of the inscription, and to arrive, I would flatter myself, at a full and complete interpretation of it. In order, therefore, to rectify all former mistakes, arising from the injury received by the three last elements from time, I shall now beg leave to transmit this short paper to you, to be laid before the Royal Society; not doubting but it will meet with the same candid acceptance, the same favourable reception, with which my other papers have been honoured, through the whole course of my correspondence with you and two of your predecessors, from that very learned and most illustrious body.

(*) Towards the close of May, 1771, the Dean sent me a draught of the Samnite coin considered in this paper, very accurately taken; soon after which, the original inscription on the reverse of the medal, by an unlucky accident, was totally defaced. However, by the assistance of the draught sent me, and my similar coin, on which the remains of the three injured letters were rendered more visible, by the help of a pretty good glass, and an attentive comparison of them with the correspondent elements on the draught the Dean communicated to me, the inscription exhibited by my medal, as well as the other given here, approaches the truth as near as possible.

II.

I am then fully convinced from the Samnite, or Samnite-Etruscan, inscription formerly visible on the reverse of the Dean's very valuable denarius, which I shall beg leave to consider here, that the true legend exhibited by my coin is **QWFIIVJ·II**, NI. LVFII, or LVVII, MER, equivalent to NI. LVFIVS, or LVVIVS, MERRISS, MERRIX, or MEDDIX. I say equivalent to LVFIVS, or LVVIVS, because the Etruscan, or Samnite-Etruscan, letter **Q** is endued with the power of V (2), as well as that of F, even (3) on this species of Samnite coins; and the Samnite termination **II** seems sometimes at least to have answered to the Roman, or Latin, IVS, as has been formerly observed. These points are clearly evinced by the authors here referred to, as well as others, that might, with equal facility, be produced. As for the elements **QW**, they very probably represented the syllable MER; nothing being more common than the suppression of a vowel between two consonants, in antient Etruscan words, as I have elsewhere demonstratively proved (4). To the instances there mentioned I might add many

(2) Anton. Francisc. *Gor. Mus. Etrusc.* Vol. II. p. 414, 415. *Philosoph. Transact.* Vol. LII. Par. I. p. 28—39.

(3) *Philosoph. Transact.* ubi sup. & Vol. LI. Par. II. p. 853—865. Anton. Francisc. *Gor.* ubi sup. & alib. Sig. Olivier. in *Sag. di Dissertaz. di Corton.* Tom. II. p. 49—73. & Tom. IV. p. 133—149. *Philosoph. Transact.* Vol. LII. Par. I. p. 34, 35. Lond. 1762.

(4) *Philosoph. Transact.* Vol. LXI. p. 85, 86, 87.

others,

others, were it in any manner necessary. But as this is not the case, it will be sufficient to refer to the paper (5) wherein the former instances are contained.

III.

That the Samnite letters **QW** stood (*) for the word MERRISS, or MERRIX, used at Herculaneum, and the neighbouring part of Campania, for MEDDIX, denoting the chief magistrate of the Oscans and Samnites, as we learn from Ennius (6) and Festus (7), will not be denied, as I apprehend, by

(5) *Philosopb. Transact.* ubi sup.

(*) The Samnites and Etruscans by a single syllable not seldom expressed a whole word on their coins. Thus for **ANVJ**, LVNA, we find **IVJ**, LVN, or **VJ**, LV, on the pieces struck in the city of LVNA; **VT**, TV, for TVTERE, on those of TVDER, or TODI; **AJT**, TLA, for TELAMON, on the medals attributed to the city going under that name, at present called TELAMONE; **IAJ**, FAI, on those of FÆSVLÆ, now denominated FIESOLE, to omit many other similar instances that occur. And as the Romans on their consular coins exhibited COS for CONSVL, DICT for DICTATOR, &c. the Samnites and Oscans on their medals probably used **QW**, MR, or MER, for MERRISS, or MEDDIX, the supreme magistrate, or rather one of the two supreme magistrates, of the city, or country, where the pieces I am considering were struck. Hence it seems to appear, that the Romans borrowed the short manner of writing here mentioned either of the Samnites, the Oscans, or the proper Etruscans. *Sag. Dissertaz. di Corton.* Tom. II. p. 53. 41. *Philos. Transact.* Vol. LIV. p. 100. 103. *Una Lettera Al Sig. Abat. Barthelemy di Annibale degli Abati Oliveri, &c.* p. 43. In Pefaro, 1757.

(6) Ennii fragm. in *Annal.* viii. Vid. etiam not. Francisc. Hesselii in loc. p. 147, 148. Amstelodami, 1707.

(7) Fest. in voc. *Meddix*.

any one who has read what has been advanced on this head by Sig. Avvocato (8) Passeri. This magistrate is called (*) Meddixuticus by (9) Livy. NI. LVFIUS, or LVVIVS, therefore, seems not to have been one of the Italian generals in the Social War, as (10) I formerly supposed, but one of the chief magistrates either of the Oscans or the Samnites, coeval with that war; there having (11) been two such magistrates, answering to the two Roman Consuls, and the two Carthaginian Suffetes, in both those nations. That D inverted, **Δ**, amongst the antient Etruscans, and the Samnites, had the power of R, whence it came to pass that these two elements were looked upon as nearly related to each other, and R not infrequently used for D, we learn from the (12) famous Matthæus Ægyptius and Father (13) Gori. We are not therefore to be

(8) Joan. Baptist. Passer. Pifaurens. *Funeral. Sacr. Mus. Herculanens.* in *Symbol. Litterar. &c.* Vol. I. p. 209, 210. Florentiæ, 1748.

(*) The Meddixuticus, or Medixuticus, of Livy seems to be formed of the two Samnite-Etruscan words **221DDEM**, or **2K1T1VT**, MERRISS, MERRICKS, or MEDDIX, TVFICKS, or TVTICKS, not TVCTICKS, as Sig. Passeri has written and pronounced it; both which words seem to denote the supreme magistrate, or rather one of the two supreme magistrates, amongst the Oscans and the Samnites. Passer. ubi sup. p. 210—213.

(9) Liv. Lib. xxvi. c. 6.

(10) *Philosoph. Transact.* Vol. LVIII. p. 258.

(11) Jo. Bapt. Passer. Pifaurens. ubi sup.

(12) Matthæus Ægypt. in *Explicat. S. G. Romanor. de Bacchanal.* p. 148, 149. Neapoli, 1729.

(13) Anton. Francisc. Gori. *Mus. Etrusc.* Vol. II. p. 412, 413. Florentiæ, 1737.

surprized

surprized that MERRISS, MERRIX, and MEDDIX, should be considered as the same word, or at least as words of the same signification, by the celebrated Sig. (14) Giovanni Battista Passeri; who has set the point I am now upon, as well as every thing he has advanced relative to it, in the strongest and clearest light.

IV.

That the Luvian, or Lufian, family was settled in that part of Campania possessed by the Oscans and Samnites, or at least almost contiguous to it, may be deemed highly probable, from the following inscription (15), found near Venafro, the antient Venatrum, at no very great distance from Herculanum, where the word MERRISS, or MERRIX, for MEDDIX, was antiently used, and now to be seen near that city (16).

LVVIA M. F. POSTVMA
SIGNVM. ET BASIM. D.
IVNONI. REG. SACRVM.

Whence, in conjunction with what has been already observed, we may conclude, that the coins I have been offering my thoughts upon may be presumed to have made their first appearance amongst either the Samnites or the Oscans, probably the former, settled in a part of Campania, at no very great distance from Herculanum; and that Ni. Lu-

(14) J. Bapt. Passer. Pisarenf. ubi sup.

(15) Ludovic. Anton. Murator. *Thesaur. veter. inscript.* &c. Tom. I. P. xvi. n. 4. Mediolani, 1739.

(16) Idem ibid.

vius, or Lufius, was the Merrifs, Merrix, or Med-dix, or at least one of the two magistrates going under that denomination, of the city, if not the whole country, where it was struck.

V.

With regard to the year, as well as the place, wherein the medals in view first appeared, neither the one nor the other can, with precision, be ascertained ; though the Latin A on one side of my denarius and the Etruscan A on the reverse, in the exergue, seem to point at the initial letter of the name of the place where they were struck. And that this operation happened about the time of the Social War, we may pronounce highly probable, from the agreement of these denarii in most particulars with other Samnite coins, undoubtedly struck not long after (*) the commencement of that war.

VI.

It would be wholly unnecessary, and altogether immaterial, to mention the different explications of the symbol on the reverses of these medals, offered by several learned men, here ; as scarce any of them seems to be perfectly agreeable to truth, or to be such as may be absolutely depended upon. I shall therefore content myself with observing, that some light is thrown upon this intricate subject

(*) This seems likewise, from what has been already observed in a former paper, pretty clearly to appear. *Philosoph. Transact.* Vol. LVIII. p. 256, 257.

by a curious passage in (17) Strabo; that the two writers who have handled this matter in the most copious, and learned, manner are Sig. (18) Bianconi and (19) M. Pellerin; that the medals I have been considering have notified to us the civil office, or dignity, of Ni. Luvius, or Lufius, as some other similar Samnite denarii have done the military post of (20) Papius Mutilus; and that the name of the Lufian family has never yet appeared, as I apprehend, on any other ancient coins. I shall only beg leave to add, that I remain, with all possible consideration and esteem,

S I R,

Your most obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.

June 18, 1772.

John Swinton.

(17) Strab. *Geogr.* Lib. v. p. 250. Lutetiæ Parisiorum, 1620.

(18) Joan. Bapt. Biancon. *de Prisc. Hebræor. et Græcor. Lit. Libel.* p. 73. Bononiæ, 1748.

(19) *Supplém. aux Recueils des Médailles.* p. 11, 12. A Paris, 1765.

(20) *Philosoph. Transact.* Vol. LI. Par. II. p. 857. Vol. LII. Par. I. p. 28—39. & Vol. LIX. p. 432—444. Lond. 1769.

V. *A Catalogue of the Fifty Plants, from Chelsea Garden, presented to the Royal Society, by the Worshipful Company of Apothecaries for the Year 1771, pursuant to the Direction of the late Sir Hans Sloane, Bart. M. D. Soc. Reg. nuper Præses: By Stanesby Alchorne, Member of the said Society of Apothecaries in London.*

Read Nov. 26, 2451
1772.

- A** C E R, *Pseudo - Platanus*, foliis quinque lobis inæqualiter serratis, floribus racemosis. Lin. Spec. plant. 1495. 2.
Acer montanum candidum. Bauh. pin. 430.
2452 *Æsculus, Hippo-Castanum*, floribus heptandris. Lin. Spec. plant. 488. 1.
Castanea folio multifido. Bauh. pin. 419.
2453 *Andromeda, Polifolia*, pedunculis aggregatis, corollis ovatis, foliis alternis lanceolatis revolutis. Lin. Spec. plant. 564. 5.
Erica humilis rosmarini foliis, unedonis flore, capsula cistioide. Pluck. alm. tab. 175. fig. 3.
2454 *Aquilegia, vulgaris*, nectariis incurvis. Lin. Spec. plant. 752. 1. α.

Aquilegia

- Aquilegia sylvestris. Bauh. pin. 144.
- 2455 Arbutus, *Andrachne*, caule arboreo, foliis glabris integerrimis, baccis polyspermis. Lin. Spec. plant. 566. 2.
Arbutus folio non ferrato. Bauh. pin. 460.
- 2456 Artemisia, *Dracunculus*, foliis lanceolatis glabris integerrimis. Lin. Sp. plant. 1189. 19.
Dracunculus hortensis. Bauh. pin. 98.
- 2457 Artemisia, *vulgaris*, foliis pinnatifidis planis incis; subtus tomentosis; racemis simplicibus, floribus ovatis, radio quinquefloro. Lin. Spec. plant. 1189. 16.
Artemisia vulgaris major. Bauh. pin. 137.
- 2458 Betula, *nana*, foliis orbiculatis crenatis. Lin. Spec. plant. 1394. 4. Flor. Lap. tab. 6. fig. 4.
Betula pumila. Laef. Pruff. 10.
- 2459 Caltha, *palustris*. Lin. Spec. plant. 784.
1. α .
Caltha palustris flore simplici. Bauh. pin. 276.
- 2460 Capficum, *annuum*, caule herbaceo, pedunculis solitariis. Lin. Spec. plant. 270. 1.
Piper Indicum vulgatiffimum. Bauh. pin. 102.
- 2461 Capficum, *frutescens*, caule fruticoso, pedunculis geminis. Lin. Spec. plant. 271.
2. β .
Capficum minus, fructu parvo pyramidali erecto. Sloan. Hist. I. 240. tab. 146. fig. 2.
- 2462 Centaurea, *benedicta*, calycibus duplicato-spinosis lanatis involucrentibus, foliis semidecurrentibus

- rentibus denticulato-spinosis. Lin. Spec. plant. 1296. 42.
- Carduus sylvestris hirsutus, f. Carduus benedictus. Bauh. pin. 378.
- 2463 Cupressus, *sempervirens*, foliis imbricatis, frondibus quadrangulis. Lin. Spec. plant. 1422. 1. a.
- Cupressus. Bauh. pin. 488.
- 2464 Daphne, *Laureola*, racemis axillaribus, foliis lanceolatis glabris. Lin. Spec. plant. 510. 6.
- Laureola sempervirens flore viridi, quibusdam Laureola mas. Bauh. pin. 662.
- 2465 Daphne, *Mesereum*, floribus sessilibus ternis caulinis, foliis lanceolatis deciduis. Lin. Spec. plant. 509. 1.
- Laureola folio deciduo, flore purpureo, officinis Laureola foemina. Bauh. pin. 462.
- 2466 Dirca, *palustris*. Lin. Spec. plant. 512. 1.
- Thymelæa floribus albis primo vere erumpentibus, foliis oblongis acuminatis, viminibus & cortice valde tenacibus. Geor. Virg. 155.
- 2467 Erica, *daboecii*, racemo terminali, foliis alternis subtus tomentosis. Lin. Spec. plant. 509. 38.
- Erica Hibernica, foliis myrti pilosis subtus in canis. Pet. gaz. 42. tab. 27. fig. 4.
- 2468 Fragaria, *sterilis*, caule decumbente repente. Lin. Spec. plant. 709. 3.
- Fragaria sterilis. Bauh. pin. 327.
- 2469 Gardenia, *Florida*. Lin. Spec. plant. 305. 1.

- Jasminum, foliis lanceolatis oppositis integerrimis, calycibus acutioribus. Miller's Icons, tab. 180.
- 2470 Genista, *Anglica*, spinis simplicibus, ramis floriferis inermibus, foliis lanceolatis. Lin. Spec. plant. 999. 11.
- Genista minor aspaltoïdes. Bauh. pin. 395.
- 2471 Heuchera, *Americana*. Lin. Spec. plant. 328. 1.
- Sanicula, f. Cortusa Americana spicata, floribus squalide purpureis. Pluk. alm. tab. 58. fig. 3.
- 2472 Hyssopus, *officinalis*, spicis fœcundis. Lin. Spec. plant. 796. 1. α.
- Hyssopus officinarum cœrulea, f. spicata. Bauh. pin. 217.
- 2473 Jasminum, *officinale*, foliis oppositis pinnatis. Lin. Spec. plant. 9. 1.
- Jasminum vulgatus flore albo. Bauh. pin. 397.
- 2474 Knautia, *orientalis*. Lin. Spec. plant. 146.
- Scabiosa orientalis, caryophylli flore. Vaill. act. 1772. p. 241.
- 2475 Ligusticum, *Levisticum*, foliis multiplicibus, foliolis superne incis. Lin. Spec. plant. 359. 1.
- Ligusticum vulgare. Bauh. pin. 157.
- 2476 Medicago, *lupulina*, spicis ovalibus, leguminibus reniformibus monospermis, caule procumbente. Lin. Spec. plant. 1097. 7.
- Trifolium pratense luteum capitulo brevior. Bauh. pin. 228.
- Vol. LXIII. F 2477 Mer-

- 2477 *Mercurialis*, *annua*, caule brachiato, foliis glabris, floribus spicatis. Lin. Spec. plant. 1465. 3.
Mercurialis testiculata, f. mas. & } Bauh. pin.
Mercurialis spicata, f. fœmina. } 121.
- 2478 *Mercurialis*, *perennis*, caule simplicissimo foliis scabris. Lin. Spec. plant. 1465. 1.
Mercurialis montana testiculata, & } Bauh. pin.
Mercurialis montana spicata. } 122.
- 2479 *Petiveria*, *alliacea*, floribus hexandris. Lin. Spec. plant. 486. 1.
Verbenæ aut *Scorodonix* affinis anomala, flore albedo, calyce aspero, allii odore. Sloan. Hist. I. 172.
- 2480 *Pœonia*, *fœmina*, foliolis oblongis. Lin. Sp. plant. 747. 1. α.
Pœonia communis, f. fœmina. Bauh. pin. 323.
- 3481 *Pteris*, *aquilina*, frondibus supradecompositis, foliolis pinnatis : pinnis lanceolatis : infimis pinnatifidis, superioribus minoribus. Lin. Spec. plant. 1533.
Filix ramosa major, pinnulis obtusis non dentata. Bauh. pin. 357.
- 2482 *Quercus*, *Ilex*, foliis ovato-oblongis indivisis serratisque, petiolatis, subtus incanis, cortice integro. Lin. Spec. plant. 1412. 3. β.
Ilex folio angusto non ferrato. Bauh. pin. 424.
- 2483 *Quercus*, *Suber*, foliis ovato-oblongis indivisis serratis subtus tomentosis, cortice ramoso fungoso. Lin. Spec. plant. 1413. 5.
Suber latifolium sempervirens. Bauh. pin. 424.
2484 Ra-

- 2484 *Ranunculus*, *Auricomus*, foliis radicalibus reniformibus crenatis incis, caulinis digitatis linearibus, caule multifloro. Lin. Spec. plant. 775. 13.
Ranunculus nemorosus, f. *sylvaticus*, folio subrotundo. Bauh. pin. 178.
- 2485 *Ranunculus*, *Flammula*, foliis ovato-lanceolatis petiolatis, caule declinato. Lin. Spec. plant. 773. 1. a.
Ranunculus longifolius palustris minor. Bauh. pin. 180.
- 2486 *Rheum*, *palmatum*, foliis palmatis acuminatis. Lin. Spec. plant. 531. 3.
Rhabarbar. Brun. orient. 192. tab. 73.
- 2487 *Rubus*, *arcticus*, foliis ternatis, cane inermi unifloro. Lin. Spec. plant. 708. 11.
Rubus caule unifloro, foliis ternatis. Lin. Fl. Lap. 207. tab. 5. fig. 2.
- 2488 *Rufcus*, *Androgynus*, foliis margine floriferis. Lin. Spec. plant. 1474.
Rufcus latifolius è foliorum sinu florifer & baccifer. Dil. Hort. Eth. 330. tab. 250. fig. 332.
- 2489 *Salvia*, *nemorosa*, foliis cordato-lanceolatis serratis planis, bracteis coloratis, corollæ labio infimo reflexo. Lin. Spec. plant. 35. 10.
Horminum sylvestre, *salvifolium minus*. Bauh. pin. 239.
- 2490 *Scutellaria*, *albida*, foliis subcordatis ferratis rugosis opacis, spicis secundis bracteis ovatis. Lin. Mantissa. 248.
Scutellaria Teucris facie flore albo. Bauh. hist. III. 291.

- 2491 *Serratula, spicata*, foliis linearibus basi ciliatis, floribus spicatis sessilibus lateralibus, caule simplici. Lin. Spec. plant. 1147. 11.
Jacca angustifolia, tuberosa radice Virginiana. Pluk. alm. tab. 424. fig. 6.
- 2492 *Sibthorpia, peregrina*, foliis reniformibus crenatis, pedunculis geminis. Lin. Spec. plant. 880. 3.
Planta anonyma. Pluk. phyt. tab. 257. fig. 5.
- 2493 *Staphylea, pinnata*, foliis pinnatis. Lin. Spec. plant. 386. 1.
Pistacia sylvestris. Bauh. pin. 401.
- 2494 *Taxus, baccata*, foliis approximatis. Lin. Spec. plant. 1472. 1.
Taxus. Bauh. pin. 505.
- 2495 *Teucrium, hircanicum*, foliis cordato-oblongis obtusis, caule brachiato dichotomo, spicis longissimis terminalibus sessilibus spirali-bus. Lin. Spec. plant. 789. 16.
Teucrium foliis cordatis crenatis petiolatis, spicis oblongis densissimis. Arduin. Spec. 13. tab. 4.
- 2496 *Teucrium, Scordium*, foliis oblongis sessilibus dentato-serratis, floribus geminis lateralibus pedunculatis, caule diffuso. Lin. Spec. plant. 790. 20.
Scordium. Bauh. pin. 247.
- 2497 *Thymus, Acinos*, verticillis sexfloris, caulibus erectis subramosis, foliis acutis serratis. Lin. Spec. plant. 826. 4.
Clinopodium avense, ocymi facie. Bauh. pin. 225.

- 2498 *Thymus, Serpyllum*, floribus capitatis, caulibus decumbentibus, foliis planis obtusis, basi ciliatis. Lin. Spec. plant. 825. 1. è.
Serpyllum foliis citri odore. Bauh. pin. 220.
- 2499 *Thymus, vulgaris*, erectus, foliis revolutis ovatis, floribus verticillato-spicatis. Lin. Spec. plant. 825. 2. α.
Thymus vulgaris folio tenuiore. Bauh. pin. 219.
- 2500 *Viola, odorata*, acaulis, foliis cordatis, stolonibus reptantibus. Lin. Spec. pl. 1324. 8.
Viola martia purpurea, flore simplici odore. Bauh. pin. 199.

VI. *Extract of a Letter from Mr. Ebenezer Kinnerley to Benjamin Franklin, L.L. D. F. R. S. on some electrical Experiments made with Charcoal.*

Received August 16, 1772.

Philadelphia, October 13, 1770.

Read Dec. 10, 1772. **T**HE conducting quality of some sorts of charcoal is indeed very remarkable. I have found oak, beech, and maple, to conduct very well; but tried several pieces of pine coal, without finding one that would conduct at all; perhaps they were made in a fire not hot enough, or not continued in it long enough. A strong line drawn on paper with a black lead pencil, will conduct an electrical shock pretty readily; but this, perhaps, may not be new to you.

On the 12th of last July, three houses in this city, and a sloop at one of the wharfs, were, in less than an hour's time, all struck with lightning. The sloop, with two of the houses, were considerably damaged; the other was the dwelling-house of Mr. Joseph Moulde, in Lombard-street, which was provided with

with a round iron conductor, half an inch thick, its several lengths screwed together, so as to make very good joints, and the lower end five or six feet under ground; the lightning, leaving every thing else, pursued its way through that, melted off six inches and a half of the slenderest part of a brass wire fixed on the top, and did no further damage within doors, or without. Captain Falconer, who brings you this, was in the house at the time of the stroke, and says it was an astonishing loud one.

VII. *Account of an Experiment made with a Thermometer, whose Bulb was painted black, and exposed to the direct Rays of the Sun: In a Letter from Richard Watfon, D. D. Regius Professor of Divinity at Cambridge, and F. R. S. to Mathew Maty, M. D. Sec. R. S.*

Hereham, September 18, 1772.

DEAR SIR,

Read Dec. 1,
1772. **D**URING the hot weather, which we had in the latter end of June and the beginning of July last, I made an experiment at Cambridge, which I then thought no more of, but which an accident hath brought to my mind again; and I now venture to send you an account of it, in hopes that some of your philosophical friends will take the trouble of prosecuting it. I exposed the bulb of an excellent thermometer to the direct rays of the Sun, when the sky was perfectly free from clouds: the mercury rose to 108° of Fahrenheit's scale, and continued stationary. A fancy struck me, to give the bulb a black covering; this was easily effected by a camel's hair pencil and Indian ink; the
mercury

mercury sunk a few degrees during the application of the coating, and the evaporation of the water; but presently after rose to 118° , or 120° in consequence of the black coat with which I had covered that part of the bulb which was exposed to the Sun. If the bulbs of several corresponding thermometers were painted of different colours, and exposed at the same time to the Sun, for a given period, some conjectures, respecting the disposition of the several primary colours for receiving and retaining heat, might be formed, which could not fail of being, in some degree, interesting.

I am,

DEAR SIR,

Your most obedient servant,

Richard Watson.

VIII. *A Report of the Committee appointed by the Royal Society, to consider of a Method for securing the Powder Magazines at Purfleet.*

TO the PRESIDENT and MEMBERS of the
ROYAL SOCIETY.

GENTLEMEN,

THE Society being consulted by the Board of Ordnance, on the propriety of fixing conductors for securing the powder magazines at Purfleet from lightning, and having thereupon done us the honour of appointing us a committee, to consider the same, and report our opinion; we have accordingly visited those buildings, and examined, with care and attention, their situation, construction, and circumstances, which we find as follows.

They are five in number, each about 160 feet long, and about 52 feet wide, built of brick, arched under the roof, which in one of them is slated, with a coping of lead 22 inches wide on the ridge from end to end; and the others, as we were informed,
are

are soon to be covered in the same manner. They stand parallel to each other at about 57 feet distance, and are founded on a chalk rock, about 100 feet from the river, which rises in high tides within a few inches of the level of the ground, its brackish water also soaking through to the wells that are dug near to the buildings.

The barrels of powder, when the magazines are full, lie piled on each other up to the spring of the arches; and there are four copper hoops on each barrel, which, with a number of perpendicular iron bars, (that came down through the arches, to support a long grooved piece of timber, wherein the crane was usually moved and guided to any part where it was wanted) formed broken conductors within the building, the more dangerous from their being incomplete, as the explosion from hoop to hoop, in the passage of lightning drawn down through the bars among the barrels, might easily happen to fire the powder contained in them. But the workmen were removing all those iron bars (by the advice of some members of this Society, who had been previously consulted); a measure we very much approve of.

On an elevated ground, nearly equal in height with the tops of the magazines, and 150 yards from them, is the house where the Board usually meet. It is a lofty building, with a pointed hip-roof, the copings of lead down to the gutters, from which leaden pipes descend at each end of the building into the water of wells of 40 feet deep, for the purpose of conveying water forced up by engines to a cistern

in the roof. There is also a proof-house, adjoining to the end of one of the magazines, and a clock-house, at the distance of feet from them, which has a weathercock on an iron spindle, and, probably, some incomplete conductors within, such as the wire usually extending up from a clock to its hammer, the clock, pendulum, rod, &c.

The blowing up of a magazine of gun-powder by lightning, within a few years past, at Brescia in Italy, which demolished a considerable part of the town, with the loss of many lives, does, in our opinion, strongly urge the propriety of guarding such magazines from that kind of danger; and since it is now well known, from many observations, that metals have the property of conducting lightning; and a method has been discovered of using that property for the security of buildings, by so disposing and fixing iron rods, as to receive, and convey away, such lightning as might otherwise have damaged them; which method has been practised near twenty years in many places, and attended with success, in all the instances that have come to our knowledge, we cannot, therefore, but think it adviseable to provide conductors of that kind for the magazines in question.

In common cases, it has been judged sufficient, if the lower part of the conductor were sunk three or four feet into the ground, till it came to moist earth; but this being a case of the greatest importance, we are of opinion that greater precaution should be taken. Therefore, we would advise, that, at each end of each magazine, a well should be dug
in.

in or through the chalk, so deep as to have in it at least four feet of standing water. From the bottom of this water should arise a piece of leaden pipe, to or near the surface of the ground, where it should be strongly joined to the end of an upright iron bar, an inch and half diameter, fastened to the wall by leaden straps, and extending ten feet above the ridge of the building, tapering from the ridge upwards to a sharp point, the upper 12 inches of copper, the iron to be painted.

We mention lead for the under-ground part of the conductor, as less liable to rust in water and moist places; in the form of a pipe, as giving greater stiffness for the substance; and iron for the part above-ground, as stronger, and less likely to be cut away. The pieces, of which the bar may be composed, should be screwed strongly into each other, by a close joint, with a thin plate of lead between the shoulders, to make the joining or continuation of the metal more perfect. Each rod, in passing above the ridge, should be strongly and closely connected by iron or lead, or both, with the leaden coping of the roof, whereby a communication of metal will be made between the two bars of each building, for a more free and easy conducting of the lightning into the earth.

We also advise, in consideration of the great length of the buildings, that two wells, of the same depth with the others, should be dug within twelve feet of the doors of the two outside magazines; that is to say, one of them on the north side of the north building, the other on the south side of the south build--

building; from the bottom of which wells, similar conductors should be carried up to the eaves, there joining well with a plate of lead, extending on the roof up to the leaden coping of the ridge, the said plate of lead being of equal substance with that of the coping.

We are further of opinion, that it will be right to form a communication of lead from the top of the chimney of the proof-house to the lead on its ridge, and thence to the lead on the ridge of the corridor, and thence to the iron conductor of the adjacent end of the magazine; and also to fix a conductor from the bottom of the weather-cock spindle of the clock-house, down on the outside of that building, into the moist earth.

As to the board-house, we think it already well furnished with conductors, by the several leaden communications above-mentioned, from the point of the roof down into the water, and that, by its height and proximity, it may be some security to the building below it; we therefore propose no other conductor for that building, and only advise erecting a pointed iron rod on the summit, similar to those before described, and communicating with those conductors.

To these directions we would add a caution, that, in all future alterations or repairs of the buildings, special care be taken that the metalline communications be not cut off or removed.

It remains that we express our acknowledgements to Sir Charles Frederick, Surveyor-general of the Ordnance, for the obliging attention with which he entertained

entertained and accommodated us on the day of our enquiry.

With very great respect, we are,

GENTLEMEN,

Your most obedient,

humble servants,

August 21, 1772.

H. Cavendish,
William Watson,
B. Franklin,
J. Robertson.

Mr.

Mr. WILSON's *Dissent to Part of the preceding Report.*

I Dissent from the Report above, in that part only which recommends that each conductor should terminate in a *point*.

My reason for dissenting is, that such conductors are, in my opinion, less safe than those which are not *pointed*.

Every *point*, as such, I consider as *soliciting* the lightning, and, by that means, not only contributing to *increase* the quantity of every actual discharge, but also frequently occasioning a discharge where it might not otherwise have happened.

If, therefore, we invite the lightning, while we are ignorant what the quantity, or the effects of it, may be, we may be *promoting* the very mischief we mean to prevent.

Whereas if, instead of *pointed*, we make use of blunted conductors, those will as effectually answer the purpose of conveying away the lightning *safely*, without that tendency to *increase* or *invite* it.

My further reasons for disapproving of *points*, in all cases, where conductors are judged necessary, are contained in a letter addressed to the Marquis of Rockingham, and published in the Philosophical Transactions, Vol. LIV. p. 247.

There are other reasons also, which I have to offer, for rejecting points on this *particular occasion*; and which, *were mentioned at the committee*. Those I shall lay before the Royal Society at another opportunity, for the benefit of the publick.

Royal Society House,
August 21, 1772.

Benj. Wilson.

IX. *Obser-*

IX. *Observations upon Lightning, and the Method of securing Buildings from its Effects: In a Letter to Sir Charles Frederick, Surveyor-General of His Majesty's Ordnance, and F. R. S. By Benjamin Wilson, F. R. S. & Ac. R. Upf. Soc.*

S I R,

YOUR station, as Surveyor-General of His Majesty's Ordnance, being such, as makes the subject of this paper particularly interesting to you, I presume an apology for this address will be wholly unnecessary.

Upon an application of the Board of Ordnance to the Royal Society, in July last, a committee was appointed, to consider of the properest method for securing the *Magazine at Purfleet* from mischief by lightning: which committee reported to the council of that learned body, what they thought necessary to be done upon that occasion. The council, afterwards, transmitted to the board, a copy of that report, together with another paper written by myself, in consequence thereof.

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For,

For, during the consideration of that business, some doubts having arisen in my mind, with regard to the propriety of *points*, which were proposed to terminate the top of each conductor; and those doubts being founded upon experiments and observations, I could not consistently subscribe to that report, nor suppress my opinion, on a subject of such importance.

Whatever may be the sentiments of others respecting those doubts, yet, they being the result of my mature consideration, I thought it my duty to propose them to the committee; and further to express my dissent, in writing, to that particular part of their report: giving, at the same time, some of the principal reasons for such dissent; and referring them, for further satisfaction on this subject, to a letter which is already published in the Transactions of the Royal Society.

As that dissent is the origin of this paper, a copy of it is here inserted. See page 48 of this Volume.

AGREEABLE to the declaration at the end of the above dissent, I shall now proceed to offer my further reasons for objecting to pointed conductors.

Experience, which is our best guide in all physical enquiries, but particularly in electrical ones, every day convinces me, that we know but little of that subtle fluid, which operates so *secretly*, and at the same time so powerfully, upon the earth, and its atmosphere. I confess that I am even now less acquainted with the principle of its action, than I thought I was twenty years ago: the *smallest differences*

ferences in the circumstances of our experiments, frequently causing very material differences in their results. And perhaps no one, who has not applied his mind closely to enquiries of this kind, could conceive how the *pointing* a piece of metal, or *not*, should make any material difference in the experiment.

The electrician has it always in his power to convince any one of the fact, who, through inexperience, may be inclined to entertain the least scruple about it: for *even from those experiments* to which it was thought proper to appeal at the committee, it appeared, that the difference in the effects upon this fluid, between *pointed* and *blunted* metal, is as 12 to 1.

A thunder-cloud therefore, according to that reasoning, (the circumstances of it being supposed to be nearly similar with what is called the prime-conductor in those experiments), if it acted at 1200 yards distance upon a point, would require a blunted end to be brought within the distance of 100 yards; and beyond those limits, would pass over it, without affecting it at all. On this occasion permit me to observe, that the *longer* the conductors are above any building, the *more danger* is to be apprehended from them; as they will in that case approximate nearer in their effects to those that are pointed. And that is one reason why I was not for advising the proposed conductors at *Purfleet*, to be so high as *ten feet* above the magazines, and more particularly upon that building called the *Board-house*, which stands considerably higher than the magazines themselves.

But, before we advance farther into this subject, it may be proper to shew the reasons for introducing a

pointed apparatus, when the experiment upon lightning was first proposed: what good consequences were derived from that experiment: and why, upon further experiments and observations, such points ought now to be laid aside, when our intention is *not to make electrical experiments*, but by the means of conductors, *to preserve buildings from the dangerous effects of lightning*.

Dr. Franklin, in his conjectures, that lightning and electricity were one and the same fluid, considered how he should *invite*, or *bring down and collect the lightning*, so as to make experiments upon it.

And he concluded, from observation, that the likeliest method would be, to make use of such an apparatus for the purpose, as was most susceptible of electric effects; or, in other words, such an apparatus as would receive the electric fluid with the greatest ease.

Repeated experiments taught him, that *metals* had the property of receiving that fluid, with more ease than other substances.

He also learnt, from the like experience, that metals *by being pointed*, were rendered still more susceptible of receiving it.

And therefore, he proposed an experiment to be tried, “ Whether it was not in our power to invite, “ or bring down the lightning, by an apparatus, “ consisting of an *electric stand*, and an iron rod, “ twenty or thirty feet in length, rising upright “ from the middle of the stand, and at the top, “ terminating in a *very sharp point*.” This apparatus was recommended to be put upon some high building,

building, with the expectation, that if a thunder-cloud should happen to pass near this apparatus, some quantity of the lightning deposited therein would probably be collected in the rod, by means of the very sharp point, and the electrical stand at the foot of the rod

That this contrivance answered the end he first proposed, we have had sufficient evidence.

And it is no wonder if, after this great discovery, we find him, and other electricians, pursuing new experiments of this kind, and raising those points higher into the air, to collect still greater quantities of that fluid which occasions lightning. Nor need we be surprized, after knowing that lightning could be brought down from the heavens by so simple an apparatus, and after experiencing its subtle effects to be similar with the electric fluid, that the Americans, and others, upon Dr. Franklin's recommendation, adopted the principle of securing their buildings from its dangerous effects, by raising above their houses rods of iron, very *sharply pointed*, and applying *wires* from the ends of those rods, down the outside of their houses, to the ground.

But though there appeared many arguments at that time in favour of such conductors, yet experiments and observations, at last, induced Dr. Franklin to *alter his opinion* in respect to those *wires*, and to substitute in their place *rods of iron*: still retaining the principle of having the rods at the top *sharply pointed*; and many of the Americans, as well as Europeans, approved of the alteration, as appeared afterwards, from constructing their conductors accordingly.

About

About that time great attention was given, and many new experiments were made, in consequence of the frequent dangerous effects, which lightning was observed to produce in some valuable buildings, by rending and splitting to pieces very large stones and timber. These were connected together by cramps and bars of iron: and at other times breaking and melting part of those rods, and sometimes exploding with, even of a considerable thickness, like so much gun powder.

From careful observations of these extraordinary appearances produced by violent shocks of lightning; and upon making other experiments relating to a certain resisting power *in, or upon*, all bodies, which appears to act against the attacks of lightning, as well as against the electric fluid, philosophers were enabled to assign the reason, and, it is apprehended, upon a solid foundation, why Conductors should be made of *metal*, in preference to all other materials; as the power of resisting such attacks is less in metals than in wood, stone, or marble.

And that this resistance might be the more simple and uniform, it appeared the most eligible to have the conductors made of *one continued piece of metal only*, and of *an equal diameter throughout*. But what that diameter ought to be, depended upon other circumstances, some of which are taken notice of in a former paper, referred to above, which I laid before the Royal Society.

By this historical sketch, we see the propriety of Dr. Franklin's introducing points, and the advantage philosophy has derived from them: by ascertaining
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that

that lightning and electricity are one and the same fluid: which appears to be diffused every where, at least upon *this earth* and in *the atmosphere*.

But when *curiosity*, which I apprehend was one of the first motives for introducing points to invite the lightning, was *satisfied*; and *experience* had taught us, that we had it in our *power* to *collect* that fluid which occasions it: and when the *principle* of its action was from experiments thus investigated and *ascertained*, this manner of *invitation*, viz. by using points, ought, in my opinion, to have *ceased* *; because a greater quantity of lightning, than we have yet experienced, may chance to attack us.

For we are so far from knowing how great the magazine of lightning may be in the heavens, or in the earth, when it is ready to discharge itself, either by one or more explosions, that we are ignorant, *even of the quantity actually discharged*, whenever any stroke from lightning visits us.

Nor can the ablest philosopher *fix the limits* of the *greatest discharge* that may possibly happen.

Seeing then how vain it is to look for any thing like *absolute security*, in all cases, it surely behoves us to proceed with *caution*. And it is for that reason I have always considered pointed conductors as being *unsafe*, by their great readiness to *collect the lightning in too powerful a manner*. And lest the conductors, without such points, should be too slender for very

* Unless where the electrician, like *Professor Richmann* (who was killed by it) at his *own hazard*, chuses to make further observations on lightning.

violent attacks, in places of great consequence, I have always recommended the having them above *four times* larger in diameter, than what are commonly made use of, to the end our security may be the greater, by opening a larger passage for any extraordinary discharge, and so far lessening the danger to be apprehended from it.

I ought not, in this place, to omit taking notice of a paper, containing some further experiments and observations, which were produced at the committee, to shew, among other things, that pointed metals were more disposed to receive the lightning, by virtue of a *repelling principle* in the lightning, as well as the electric fluid, which acted upon the *natural quantity* of the fluid contained within the metal, at a considerable distance from the *point*, causing, if I may be allowed the expression, a kind of *vacuum* therein; but I suppose the author means to a certain distance only.

So far from disputing this philosophy, I readily admit the fact.

But, I am afraid, *every attempt* to prove that pointed conductors may be so disposed to receive this fluid more readily, will not mend the argument in the least; because, the more we lessen the power of resisting, even supposing the whole Conductor to be in that state, the more we increase the power of invitation.

In regard to other experiments, with *locks of cotton* *, which are acted upon in a particular manner

* Dr. Franklin's Experiments.

by the apposition of points, and the conclusions drawn from thence, in favour of pointed conductors, as causing similar effects upon the *fragments or small clouds*, which, hanging below the thunder-clouds, have been supposed a kind of *stepping-stones*, for the lightning to pass upon, towards the earth: such pointed conductors being supposed to occasion those *fragments* to retire up into the cloud from whence they were suspended; and on that account, to prevent a stroke from lightning, which might otherwise have happened, I shall, for the present, wave entering into this philosophy, as I could wish the conjecture to be reconsidered; because I apprehend it is liable to many objections, which to enumerate would carry me beyond the proper bounds of such a paper as this. However, if the same opinion should again be offered, and brought in argument, it may be worth while to enter more *deeply* into the enquiry.

If those gentlemen, who argued at the committee for the *necessity of points*, could have made it appear, that such points draw off, and conduct away, the lightning *imperceptibly* and *by degrees*, without causing any explosion, during a thunder storm (which seems to have been once the opinion of Dr. Franklin) I should readily have subscribed to their Report.

But experience shews us, that the fact is otherwise: there being many instances, where violent explosions of lightning have happened to *conductors* that were *sharply pointed*. And *three* in particular, the accounts of which are inserted in a publication of Dr. Franklin's *, where the *points*

* Dr. Franklin's Experiments, p. 394. 416, 417, &c.

were *dissipated*, or *destroyed*; and a small part of an iron rod melted next the points of one of them; and also at the several crooked ends of the rods below, where they were hooked on to each other, and formed the conductor belonging to Mr. Maine in North America. But as those letters are long, and contain several other curious facts, I shall reserve them, together with some further observations upon the *nature* and *power* of that *resisting principle*, which is found to act so sensibly against the attacks of the electric fluid, or lightning, to some future dissertation.

There is no building, that I know of, more exposed to this kind of danger, than the *Eddystone Lighthouse*, as it stands upon a rock in the sea, several miles from land. The fixing of a conductor to that building, was thought highly proper; and the fixing of a *point* upon it, as highly improper. It was therefore resolved upon to put up a *conductor without a point*, that no more lightning might be unnecessarily solicited to the building, and that all the lightening, which accidentally fell on it, might be conveyed away without injuring it. This conductor was *fixed* twelve years ago, and the building has since received no injury from lightning*.

There is another edifice of great consequence, I mean *St. Paul's Church*, which stands much exposed, from its height, to accidents by lightning. The dean and chapter of that cathedral, thought it an object deserving the serious attention

* N. B. A former building erected for the same purpose, upon this rock, was set on fire by lightning.

of the *Royal Society*. A committee was therefore appointed, in consequence of their application : and proper *conductors* were put up, in the several places where they were thought necessary, from the top of the *lanthorn* to the *sewers* underground. And notwithstanding particular care was taken, to have the additional metal either of a considerable diameter, or an equal quantity of it turned into other shapes, for the conveniency of the several places ; yet part of those conductors, consisting of iron, in the *stone gallery*, shewed marks of their having been made *considerably hot*, if not absolutely *red*, by a stroke of lightning which happened in March last (as appears by a letter which I communicated to the *Royal Society* from one of the vergers of that church, Mr. Richard Gould) who had examined the conductors the morning following, along with Mr. Burton of the same cathedral *, and that the appearances were in general,

* Mr. Gould acquaints us in his letter, that he examined the four conductors in the lanthorn and stone gallery of St. Paul's Church, the morning after the lightning happened. That no marks whatsoever appeared upon the conductor to the *South*, which was the first he attended to. That he examined next the conductor to the *West*, and observed a *thick rust* lying upon the pavement in the stone gallery, as if it had been cleaned off, from the conductor, with a tool : that several parts of the iron appeared *black*, particularly the screws or nuts : something like the effects left by gun-powder upon iron or steel, or a smoaky fire.

That the conductor to the *North*, shewed no marks, no more than that to the *South*.

But that upon examining the conductor to the *East*, he found stronger marks *abundantly*, than on the *West* conductor,

ral, as the verger's letter related them to me. Mr. Delaval and myself attended, about a week afterwards, to observe them, and their particular

it being much blacker; particularly on the nuts and screws: the rust lying in greater quantities on the pavement. *And the extreme part of the conductor that goes into the water trunk, seemed like a piece of iron newly taken out of a forge by a smith, without working it on the anvil.*

N. B. Mr. Gould has since added to the account in his letter, some circumstances which I apprehend ought not to be omitted. He says, that where the end of the conductor, on the East side, points towards the water trunk, a stone surrounds part of it, leaving an interval, half an inch wide or more, between them, and about four or five inches long, which is a little more than the breadth of the conductor. That this interval was *filled up with dirt*, and had been so for some time, occasioned by frequent showers of rain washing the pavement in the stone-gallery. That, after the lightning happened, he observed a *hole* was made through the dirt, *one quarter of an inch in diameter*, and about *two inches in length*. That the hole was close to the iron; and that, upon stooping down his head, he perceived a very disagreeable *smell of sulphur* from the stone, dirt, and conductor, particularly the last.

Upon hearing this account, Mr. Delaval and myself, a few days ago, went and examined the conductors again; but more carefully than before. For, upon causing the stone to be removed, which covered the top of the water trunk, we had an opportunity of examining near two feet more of the iron which points to the water trunk, than we could perceive before this stone was removed. When we observed, that the conducting iron did not touch the lead. We likewise observed, that there was a *very thick coat of rust* all over that part of the iron; *particularly at the end next the lead*, where the water entered the trunk.

As the necessity of attending to these circumstances will be obvious to any one, who is but in the least degree acquainted with these researches, *the danger of neglecting them will be seen in the strongest light, by the gentlemen of the committee* who recommended the conductors for the security of that Cathedral.

situations,

situations ; with the circumstances attending them, when we were very well satisfied with his account, notwithstanding it had rained in the *interim* for three days together.

It is worthy of note, that those conductors did *not terminate in a point*, nor was any *point put* upon the *cross at the top*.—And yet Dr. Franklin was of that committee.

If points are so essential to our safety, why was not the reason enforced at the committee, for having them on that capital edifice ? For my part, I think it was a happy circumstance, that there was *no point* fixed upon the top of the church, to solicit a greater quantity of lightning at that moment, than what fell upon the conductors, circumstanced as they were : as *that quantity* was great enough to *beat so considerably a bar of iron*, near *four inches broad*, and about *half an inch thick*.

This powerful effect, reminds me of another instance still more extraordinary, which happened in Martinico, and is related by *Captain Didden*, where a bar of iron, *one inch in diameter*, was by a violent shock of lightning *reduced in one part of it, to the thickness of a slender wire only*. See Ph. Tr. Vol. LIV. p. 251.

Since then we are at all times ignorant of the quantity of *lightning in the earth* and its atmosphere ; and the difference in the effects, between blunted and pointed, ends, in causing a discharge in our electrical experiments, appears to be as *one to twelve* ; it is easy to comprehend the very great danger this noble fabrick has probably escaped, by having no pointed apparatus upon it.

From

From the above observations, I am naturally led to consider a part of the proceedings of the committee, respecting the *magazines* at *Purfleet*; when a certain number of conductors, with tapering points at the top, were resolved upon, as necessary, to protect the several buildings where the powder is deposited. For it was *agreed* upon at the same meeting, that the *Board-house*, which is a large building for the use of the board-officers, and which stands considerably higher than the magazines, as was observed above, *did not require any point at the top*: because it was apprehended to be *perfectly secure*, by reason of the copings on the roof, the gutters and pipes to carry off the water, being all of lead: and further, because those pipes communicated with two wells, which always contained water.

I was not a little surprized at this last resolution, which appeared to be so inconsistent with the former. Because, if points were necessary in one place, they ought to be so in another. And on the other hand, if the Board-house is secure by the leaden accidental conductors, which have no points, why ought not the magazines to be equally secure, when put into the same circumstances?

I therefore enforced the *inconsistency* of such a resolution in the strongest terms. Notwithstanding which, the gentlemen, at that time, thought proper to confirm their resolution. However, at the next meeting of the committee, I observed that they had been pleased, in the mean time, to make an amendment in favour of points for the board-house:

house ; which amendment was no sooner proposed, than approved of.

Why my observation was rejected at the preceding meeting, I must leave to the judgment of others. But it certainly carries an appearance, as if *manifest contradiction*, upon further reflection, must have been the *cause* of that alteration.

And I am inclined to believe, from some gentlemen of the committee expressing their opinion, “ *of its being a matter of more indifference whether blunted or pointed conductors were made use of,*” that they have not considered this subject, with all the due attention which so important an object deserves.

For if our experiments shew, that *points*, from the *nature* of their *shape*, and *other* circumstances attending them, resist the attacks of this fluid less than blunted ones ; and that blunted conductors, of proper dimensions, are sufficient to convey away the lightning *safely*, whenever it attacks them ; why should we have recourse to a method, which is at best uncertain ; and which some time or other may be productive of the most fatal effects ?

But perhaps no argument can be brought with more force against the principle of points, than Dr. Franklin’s own words, which are published in his experiments, p. 481, where he declares *positively*.
 “ Buildings, that have their roofs covered with
 “ lead, or other metal, and spouts of metal continued from the roof into the ground to carry off
 “ the water, *are never hurt by lightning* ; as whenever it falls on such a building, it passes in the
 “ metals, and not in the walls.”

This

This is the case with the *British Museum*, a building also of considerable consequence, where there are no other *conductors*, than what are formed by the *copeings, gutters and pipes*, which are all of lead, and communicate with the ground. Now it is from the great quantity of metal contained in the several pipes, together with the other circumstances attending them, that I considered that building (in a former paper laid before the royal society) as being sufficiently secured, from those dangerous accidents.

But if any gentleman should be disposed to entertain a doubt about it, or indeed of any other part of my reasoning on this subject, a *declaration* of those doubts may be attended with good consequences, as they will necessarily open the door to a more minute investigation.

I have now, Sir, gone through the reasons which I proposed to lay before the royal society for the rejecting of points. And I am very sorry, in the course of this letter, to have been under the necessity of mentioning any differences in opinion, which passed between the members of the committee, to whom this important matter was referred. I think, however, I shall stand excused to the society, and the public, when it appears, as I hope it now sufficiently does, what my motive has been; namely, to state clearly, and impartially, the objections which I conceived to lye against pointed conductors: and to disclose *without any reserve*, the principles on which such objections are grounded.

I am, S I R, with the greatest respect,
Your most obedient, and most humble servant,

8th December, 1772,
Great Russell-street,
Bloomsbury.

Benjamin Wilson.
P. S.

P. S. Mr. Delaval, who was one of the committee, has given me leave to insert his opinion upon this subject ; which is this. That he concurs with me in thinking that such conductors as are elevated higher than the buildings to which they are applied, or are pointed at the top, are *improper* and *dangerous*.

He was desirous of delivering his opinion at the committee: but, as the meetings of it were held in the summer only, his absence from London prevented his attendance.

X. *A Letter to Sir John Pringle, Bart.
Pr. R. S. on pointed Conductors.*

S I R,

Read Dec. 17, 1772. **H**AVING heard and considered the objections to our Report, concerning the fixing pointed Conductors to the Magazines at Purfleet, contained in a letter from Mr. Wilfon to Sir Charles Frederick, and read to the Royal Society, we do hereby acquaint you, that we find no reason to change our opinion, or vary from that Report.

We have the honour to be,

S I R,

Your most obedient,

humble servants,

December 17, 1772.

H. Cavendish,
W. Watfon,
B. Franklin,
J. Robertfon.

XI. *Astro-*

XI. *Astronomical Observations made at
Chislehurst in Kent, by the Reverend
Francis Wollaston, F. R. S.*

Received November 12, 1772.

Read Dec. 17, 1772. **I** the last year did myself the honour to deliver to this Society, an account of the going of an astronomical clock with a wooden pendulum, for the year preceding; together with such observations as I had made in this place; the latitude of which is $51^{\circ} 24' 33''$ North, and its longitude $4' 39'' = 18'',6$ in time, East of the Royal Observatory at Greenwich. As that account seemed not wholly unacceptable, I have now continued it down to the present time: omitting, however the series of transits; which I gave in the former paper, merely to shew the exactness with which they might be taken with a very small instrument, beyond what I had expected to find.

The rate of the clock deduced from the observations of this last year, will not be found so uniform as the foregoing. To what cause to ascribe it, I am not certain. I think not to heat: perhaps to the great drought of the summer. However, its acceleration or retardation was not desultory, but sufficient

ficient to be depended upon for any intermediate time. The clock was cleaned in November; and when set up again, lost, between the 18th and 28th, at the rate of $7''$,8 per day. The regulator was then altered, and clock set, and from that time never meddled with.

My observatory (if it deserves that name) is a room up two pair of stairs, in a square compact brick house, pretty much exposed indeed to wind, from our high situation. The face of the clock turns due East. I have set down the *throwing-out* of the pendulum, as judged by the eye, on a scale behind it divided only to every 10 minutes; and therefore some of the variations may only be errors in judgment. How far a clock so situated may be affected by any motion of the house in windy weather, deserves consideration. I do not however perceive that it is, either in the *rate* or *throwing-out*.

1771.		Clock + too fast — too slow for mean fol. time.		Gain + or Loss —	Num- ber of days.	Rate per day.	Throwing out on the	
		'	"				South side.	North side.
				"		"	o	'
Nov.	29	Clock	— 0,6	— 17,3	17	— 1,02	1 42	1 45
Dec.	16	—	17,9	— 12,1	14	— 0,86	1 43	1 46
	30	—	30,0	— 8,0	14	— 0,57	1 43	1 45
1772.								
Jan.	13	—	38,0	— 6,9	22	— 0,31	1 43	1 46
Feb.	4	—	44,9	— 9,5	11	— 0,86	1 44	1 46
	15	—	54,0	— 8,9	17	— 0,52	1 42	1 44
Mar.	3	—	1 2,9	— 2,8	32	— 0,09	1 42	1 44
April	4	—	1 5,7	+ 2,0	9	+ 0,22	1 42	1 44
	13	—	1 3,7	+ 48,3	19	+ 2,54	1 38	1 40
May	2	—	1 5,4	+ 42,8	11	+ 3,89	1 43	1 45
	13	+	27,4	+ 43,8	10	+ 4,38	1 42	1 44
	23	+	1 11,2	+ 41,8	9	+ 4,66	1 42	1 44
June	1	+	1 53,0	+ 61,1	14	+ 5,08	1 43	1 45
	15	+	2 54,1	+ 54,8	18	+ 6,38	1 43	1 45
July	3	+	4 48,9	+ 50,5	7	+ 7,21	1 43	1 45
	10	+	5 39,4	+ 81,1	11	+ 7,37	1 43	1 45
	21	+	7 0,5	+ 78,3	10	+ 7,83	1 43	1 45
	31	+	8 18,8	+ 58,1	8	+ 7,26	1 48	1 50
Aug.	8	+	9 16,9	+ 60,0	9	+ 6,44	1 47	1 49
	17	+	10 16,9	+ 72,9	11	+ 6,63	1 47	1 49
	28	+	11 29,8	+ 52,8	10	+ 5,28	1 44	1 47
Sept.	7	+	12 22,6	+ 31,3	8	+ 3,91	1 46	1 48
	15	+	12 53,9	+ 22,0	6	+ 3,33		
	21	+	13 15,9	+ 37,1	10	+ 3,71	1 48	1 50
Oct.	1	+	13 53,0	+ 27,6	9	+ 3,06		
	10	+	14 20,6	+ 20,0	9	+ 2,22		
	19	+	14 40,6	+ 32,4	14	+ 2,31	1 44	1 48
Nov.	2	+	15 13,0					

I have this year kept a register of the Thermometer and Barometer; the highest and lowest state of which, as I observed them in each month, I will here subjoin. I do not find that they have any reference to the rate of the clock; but they may serve for comparison with other places. The Thermometer A was made by Mr. Nairne; and hangs without doors, near a window, up one pair of stairs, on the North side of my house; with a screen to keep off the morning sun in summer, but no building near it. The three first columns shew its state at eight o'clock in the morning, two in the afternoon, and eleven at night. The fourth column gives the Thermometer B, in the observatory, near the face of the clock, at nine in the morning. The fifth shews the Barometer; which being portable, I cannot be certain that its scale is exactly placed; though I believe it is right. It is kept in a room on the ground floor.

1771.		Thermometer A.			B.	Baro- meter.
		H. 8. Mat.	H. 2. P. M.	H. 11. P. M.	H. 9. Mat.	
Dec.	Highest	52	50	53	52	30,05
	Lowest	34	35	33	36	28,58
1772.						
Jan.	Highest	49	52	49	50	30,07
	Lowest	22	30	21	29	28,63
Feb.	Highest	50	47	48	50	29,91
	Lowest	25	32	24	32	28,79
Mar.	Highest	51	56	46	50	29,78
	Lowest	27,5	31	27	30	28,79
April	Highest	51	59	50	52	30,04
	Lowest	31	38	31	36	29,19
May	Highest	63	70	53	58	30,20
	Lowest	42	47	33	44	29,35
June	Highest	73	82	68	73	30,12
	Lowest	53	57	49	56	29,50
July	Highest	70	76,5	63	68	30,10
	Lowest	55	61	45,5	58	29,10
Aug.	Highest	73	75	67	70	30,08
	Lowest	52	62	50	60	29,23
Sept.	Highest	66	72	66,5	70	29,96
	Lowest	43	58	46	45	28,82
Oct.	Highest	60	66	59	62	30,14
	Lowest	44	52	43	51	28,88

Eclipse of the Sun, ☉ October 25, 1772.

App.time.

h ' "

20 31 12 The first impression that I perceived. But I had looked for it on the Western instead of the Eastern edge, owing to a mistake in the Almanac; so that the Eclipse was now about its greatest obscuration. A remarkable protuberance on that part of the Moon's limb.

20 36 34 End of the Eclipse. Observed with a $3\frac{1}{2}$ feet Achromatic Telescope, magnifying 150 times.

Eclipses of Jupiter's satellites. Observed with same telescope, magnifying 100 times.

1772.		App. time.			
		h	m	s	
h	July 11.	11	22	34	First sat. im. Good.
h	27.	9	38	2	First sat. im. Vapours.
z	29.	10	29	59	Second sat. im. Cloudy and doubtful.
D	Aug. 3.	11	32	48	First sat. im. Good.
h	22.	9	38	35	Third sat. em. Hazy.
⊙	23.	10	35	19	Second sat. em. Doubtful. Flying cloud.
♀	28.	8	28	14	First sat. em. Very doubtful.
⊙	Sept. 27.	10	53	35	First sat. em. Doubtful.
⊙	Oct. 4	9	57	14	Third sat. em. Good.
		12	50	41	First sat. em. Good.
♂	13	9	17	13.5	First sat. em. Good.
♂	20.	11	14	32	First sat. em. Doubtful.

Occultations of Jupiter's satellites, and transits over his disc, and conjunctions of the satellites, and appearances of his belts. Observed with the same telescope, magnifying 150 times.

1772.		App. time.			
		h	m	s	
♀	Aug. 7.	10	44	40	Second sat. on the limb of J., in its egress from his disc.
♂	11.	10	55	50	First sat. in contact with J.'s limb.
		11	1	40	——— Total ingress on the disc. The shadow precedes the satellite.
z	12	9	30	0	Appearance of J.'s belts, as in Fig. I.
		10	45	0	Southern belt complete.
J	13	10	15	0	Appearance of J.'s belts, as in Fig. II.
♀	21.	9	13	8	Conjunction of first and second.
h	29.	9	2	0	Third sat. seemingly in contact with the disc.
		9	13	0	——— Occult.
⊙	30	9	45	0	Second sat. partly covered by the limb.
		9	48	40	——— Occult.

1772.	App. time.	
	h ' "	
2 Sept. 7.	9 38 0	Appearance of \mathcal{L} , with the shadow of the fourth fat. central. The fat. itself not visible. Fig. III.
	11 10 0	The Southern belt equally advanced with the shadow.
8	12 8 39 20	Appearance of \mathcal{L} , and shadow of the first fat. central, Fig. IV. The shadow much smaller than that of the fourth.
	9 24 30	Southern belt advanced half way.
	9 29 0	Sat. now off the disc, and shadow still on; about equidistant from the limb.
	9 40 46	Shadow going off, in contact with the limb.
3	15. 7 27 0	Appearance of \mathcal{L} , as in Fig. V.
	8 22 20	Southern belt complete, and dark spot (α) in the large belt, central.
	8 27 20	Second fat. on the limb.
	8 30 20	———— Total ingrefs on the disc.
	10 41 0	Shadow advanced about $\frac{1}{3}$ on the disc: not very visible. preceded by another dark round spot, smaller; I know not what. Fig. VI.
4	16. 8 36 0	Shadow of the third fat. about its own diameter from \mathcal{L} 's limb. Well defined.
	9 19 0	Third fat. quite off the disc. The shadow was then advanced about $\frac{1}{3}$.
	9 33 0	Southern belt reduced to about $\frac{1}{3}$. The shadow advanced about $\frac{2}{3}$. Fig. VII.
	9 40 0	Shadow central.
	11 2 30	Shadow advanced about $\frac{4}{5}$. Southern belt then scarce visible on the East side.
	11 35 0	Shadow either gone off, or not visible on the limb. Southern belt now come on $\frac{3}{4}$ or more. Vide Fig. VIII.
5	15. 8 43 30	First fat. seemingly in contact with \mathcal{L} 's limb.
	8 52 0	———— Total ingrefs.
	9 7 40	Conjunction of second and third fat.
	9 32 0	Shadow of first fat. half entered.
	9 35 30	———— totally entered.
	10 58 0	———— advanced about $\frac{2}{3}$.
	1 1 0	First fat. on the limb.
	11 4 30	———— external contact. Vide Fig. IX. Shadow very small, but well defined.

1772.	App. time	
	h' ' "	
☉ Sept. 27.	7 37 30	First sat. in contact.
	7 42 0	—— occult.
24 Oct. 1.	8 9 26	Conjunction of third and fourth sat.
	8 14 45	Second sat. in contact.
	8 20 0 ::	—— occult.
☉ 4	9 27 40	First sat. in contact.
	9 32 30	—— occult.
☾ 5	8 29 45	First sat. in contact, just gone off the l.m.b. The shadow at that time about $\frac{1}{4}$.
♂ 20.	7 40 15	First sat. in contact.
	7 44 20	—— occult.

These last observations are not to be depended upon, within several seconds; and yet, if pursued regularly, might have their use. I was led into making them from what M. Messier transmitted to this Society, in a former paper, Vol. LIX. N^o xxv. Indeed the hint he there gave, concerning the superiority of achromatic telescopes above all others for these purposes, induced me to procure one from Mr. Dollond, of his last construction, with a triple object-glass; which, I believe, is excellent in its kind: at least, it has fully answered the highest expectation I had formed of them.

The drawings shew how Jupiter appeared to me at different times. The broad belt across the middle, I cannot tell what to make of; sometimes it appears as a combination of several smaller ones; but usually clouded, or waved, in the middle, with its edges in general darkest; perhaps from their opposition to the contiguous parts, which are always much brighter than the rest of the planet. There have been two places in that belt particularly dark (especially that

marked α , Fig. V.), which I have seen latterly, whenever that side of ζ has been turned towards us; though I did not perceive them August 12, when I took Fig. I. The Northern belt, I believe, is continued uniformly round the planet; the Southern one reaches little above half way. They are both of them darker, I think, than the general complexion of the largest. There is beyond each of these, a light part, not equally bright with those zones near the middle; and from thence a gradual shading off to the poles themselves; particularly to the North. The drawings are, as the objects appear in an astronomical telescope, and therefore inverted.

Chislehurst,
November 10, 1772.

Francis Wollaston.

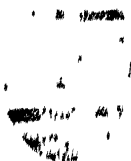
THE Astronomer Royal, who was present at the reading of this paper, expressed a wish that a particular account of the mechanism of the clock itself, might accompany the register of its going; as no consideration ought to be omitted which might serve to elucidate these matters.

My clock is in general of the plain kind, moving in brass pivot-holes, beating dead seconds, with the common steel paletts; but of good workmanship, as being made by Mr. Holmes. The rod of the pendulum is of deal, as mentioned in a former paper; to which the ball itself (weighing about fourteen pounds) is screwed fast, there being a smaller weight underneath for a regulator. Its suspension is somewhat particular. The spring A is not hung in a slit as is usual; but is fastened to a transverse piece B, on which it rests upon the sides of the cock. The shoulders of this piece confine the pendulum from any lateral motion, as much as it would be in a slit; but it is at full liberty to hang perpendicularly under its point of suspension, without any strain on either edge of the spring above the other. The crutch is also of an unusual make. The bottom of the stem, instead of receiving the crutch-pin, is turned sideways; at right angles to itself, but parallel to the back-plate. This piece D reaches about an inch, and at the end has a smooth steady joint E (known to the workmen by the name of a socket and stud), whose axis lies horizontal. From that joint there is a return F, of equal length with the former piece; at the end of which is the crutch-pin G, nearly coinciding with the end of the stem.

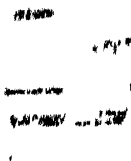
This

This return together with the crutch-pin, if the pendulum were removed, would fall down as represented by the dotted lines *f*, *g*. The design of this mechanism is that any friction at the crutch-pin, which in the common construction is a small sliding up and down at every oscillation, is hereby converted into a smaller circular motion in a smooth pivot-hole. Both these, I understand, are contrivances of the late Mr. Henry Hindley of York: and may be seen in the regulator at Mr. Holmes's in the Strand, which is constructed in the same manner. The cock is fastened to the back-plate of the clock itself; but the clock-case is made stronger than usual, and is firmly screwed to the wall, independent on the floor.

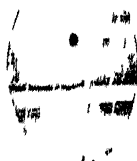
Fig I
Aug 12 h 7 32
h 9 30 0



II
Aug 13
h 10 15 0



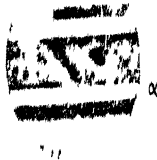
III.
Sept 7 h 9 38 0
Shadow of 4 1/2 lat



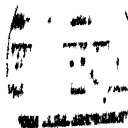
IV
Sept 12 h 8 39 20
Shadow of 1 1/2 lat



V
Sept 15 h 7 27 0



VI
Sept 15 h 10 41 20
Shadow of 2 1/2 lat



VII.
Sept 16 h 9 00 0
Shadow of 1 1/2 lat.

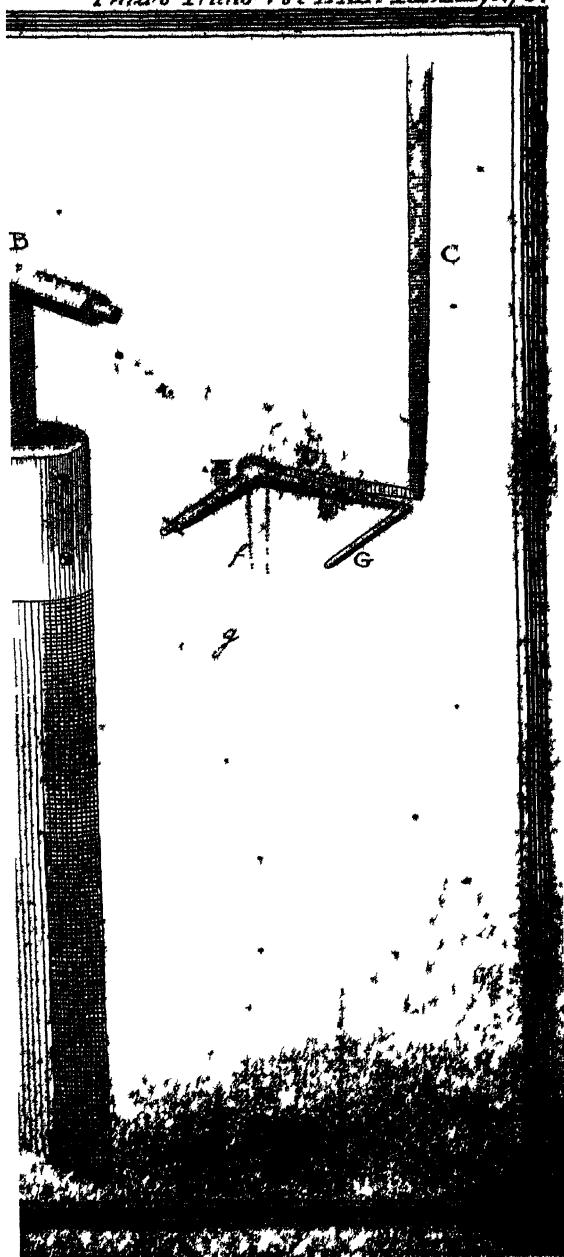


VIII.
Sept 16 h 11 35 0



IX
Sept 19 h 11 4 30
1 1/2 lat. 8 1/2 lat. & shadow





XII. *A Letter from Dr. DuRoiel, F. R. S. and F. S. A. to William Watson, M. D. F. R. S. upon the early Cultivation of Botany in England; and some Particulars about John Tradescant, a great Promoter of that Science, as well as Natural History, in the last Century, and Gardener to King Charles I.*

S I R,

Read Nov. 2, 1772. **Y**OUR love for Botany, and your great knowledge in that science, will, I hope, be a sufficient apology for laying before you some observations which have occurred to me on that subject. And I flatter myself that the following anecdotes will not be altogether unacceptable to you.

The sciences, we know, are subject to revolutions. But is it not a very extraordinary one that Botany, so useful to mankind, and so well known to the ancients, should, for some ages, abandon Europe, and remain almost unknown there till the sixteenth century; when it is supposed to have suddenly revived; and has since, by the industry of the moderns, been brought to the highest perfection?

The truth, however, is, that Botany returned into England long before this era. It was brought back here by the Saxons; since whose time, Lihall endeavour to shew, that it hath always flourished, more or less, in this Kingdom.

I found my opinion upon the authority of the four following Saxon manuscripts.

Two in the Bodleian Library, viz.

- (a) N° 4125. Herbarium Saxonicum.
- (b) N° 5169. Liber Medicinalis MS. continens Virtutes Herbarum Saxonice.

And two others in the Harleian Library, viz.

- N° 5066. entitled, Herbarium Saxonice.
- N° 585. Tractatus qui ab Anglo-Saxonibus dicebatur LIBER MEDICINALIS: scil. L. Apuleii Madaurencis Libri de Virtutibus Herbarum, Versio Anglo-Saxonica.

This Lucius Apulcius of Medaura was a famous Platonic philosopher, who flourished about A. D. 200.

From this time I have met with no MS. concerning Botany, till the thirteenth century, when (c) Bishop Tanner mentions three MSS. on this subject, written by Gilebertus Legleus, five Anglicus, a phy-

(a) Cat. MSS. Angliæ, p. 185.

(b) Ibid. p. 562.

(c) Bibliotheca, p. 474.

fician,

fician, who flourished in the year 1210, entitled,

1. De Virtutibus Herbarum, MS. Bodl. Digb. 75.
2. Gilberti Liber de Viribus & Medicinis Herbarum, Arborum, & Specierum, MS. olim Monast. Sion.
3. De Re Herbaria, Lib. I.

(d) The Bishop likewise mentions one John Arden, a famous surgeon, who lived at Newark in Nottinghamshire from 1349 to 1370, as the author of a MS. (now extant in Sir Hans Sloane's library), entitled, *Volumen Miscellaneorum de Re Herbaria, Physica, & Chirurgica.*

In the Ashmolean Library are the following MSS. viz.

- (e) (N^o 7704.) entitled, *A Treatise of Chirurgery, with an Herbal, &c.* in Old English, 4to. 1438.
And another,
(N^o 7709) called, *An Herbarie, &c.* written alphabetically, according to the Latin names, in 1443. And
(N^o 7537.) entitled, *A Book of Plants and Animals, delineated in their natural colours on velvet,* Old English, A. D. 1504.

Mr. Ames, in his *Typographical Antiquities*, p. 470, informs us, that, in the year 1516, a folio, entitled, "*The Greate Herball,*" was printed in

(d) *Bibliotheca*, p. 48.

(e) *Cat. MSS. Angl.* p. 341.

Southwark by Peter Treveris; and this, Sir, I believe, is the oldest English herbal now extant in print.

To come to later times. The ingenious Mr. Gough (in his *British Topography*, p. 61.) informs us, "That, before the year 1597, John Gerrard, citizen and surgeon of London, seems to be the first who cultivated a large physic garden, which he had near his house in Holborn, where he raised 1100 different plants and trees." (He might have add, that Gerrard had another physic garden in Old-street, containing a great variety of plants; a printed catalogue of which is to be found in the libraries of the curious). But Gerrard had a famous cotemporary, who greatly advanced that valuable science, and of whom but little hath hitherto been said by the modern biographers.

John Tradescant is the person meant. And I hope, Sir, that an attempt to revive the memory of this once eminent botanist and virtuoso will not be displeasing.

John Tradescant was, according to Anthony Wood, a Fleming, or a Dutchman. We are informed by Parkinson, that he had travelled into most parts of Europe, and into Barbary; and, from some emblems remaining upon his monument in Lambeth church-yard, it plainly appears that he had visited Greece, Egypt, and other Eastern countries.

In his travels, he is supposed to have collected not only plants and seeds, but most of those curiosities of every sort, which, after his death, were sold by his son to the famous Elias Ashmole, and deposited in his Museum at Oxford.

When

When he first settled in this kingdom, cannot, at this distance of time, be ascertained; perhaps it was towards the latter end of the reign of Queen Elizabeth, or the beginning of that of King James the First. His print, engraven by Hollar before the year 1656, which represents him as a person very far advanced in years, seems to countenance this opinion.

He lived in a great house at South Lambeth, where there is reason to think his museum was frequently visited by persons of rank, who became benefactors thereto: among these were King Charles the First (to whom he was gardener), Henrietta Maria his Queen, Archbishop Laud, George Duke of Buckingham, Robert and William Cecil, Earls of Salisbury, and many other persons of distinction (*f*).

John Tradescant may therefore be justly considered as the earliest collector (in this kingdom) of every thing that was curious in Natural History, viz. minerals, birds, fishes, insects, &c. &c. He had also a good collection of coins and medals of all sorts, besides a great variety of uncommon rarities. (*g*) A catalogue of these, published by his son, contains an enumeration of the many plants, shrubs, trees, &c. growing in his garden, which was pretty extensive. Some of these plants are (as I am informed), if not totally extinct, at least become very uncommon, even at this time. A list of some remarkable ones

(*f*) See a list of them at the end of *Museum Tradescantianum*, 12^{mo}, London, 1656—where are Hollar's two prints of John Tradescant, the father and son.

(*g*) Ibid,

introduced by him, is inserted below (*b*). And this able man, by his great industry, made it manifest (in the very infancy of Botany), that there is scarce

(*b*) From *Parkinson's Garden of pleasant Flowers*, printed in 1656.

1. " *Pseudonarcissus aureus maximus flore pleno, five roseus*
" *Tradescanti*.
" The greatest double yellow bastard daffodil, or John
" *Tradescant's* great rose daffodil.
" This daffodil was primarily introduced by John Tradescant, and for its extreme beauty, may well be entitled
" the glory of daffodils." Page 102.
2. " *Moly Homericum, vel potius Theophrasti*.
" The greatest moly of Homer. 141.
3. " *Moly Indicum, five Caucasum*.
" *Indian moly*. *ibid*.
" Both the above molys are natives of Spain, Italy, and
" Greece, and were procured from thence by John Tradescant, and flourished with him, in his garden at Canterbury," (should be South Lambeth).
4. " *Ephemerum Virginianum Tradescanti*.
" John Tradescant's spider-wort of Virginia.
" This spider-wort is of late knowledge, and for it the
" Christian world is indebted unto that painful industrious searcher and lover of all nature's varieties John
" Tradescant." 152.
5. " *Gladiolus Byzantinus*.
" Corn-flag of Constantinople.
" With this species John Tradescant observed many acres of
" ground in Barbary overspread. 190.
6. " *Elleborus albus vulgaris*.
" White hellebore.
" This groweth in many places in Germany, and also in
" some parts of Russia, and in such plenty, that John
" Tradescant observed quantity sufficient to load a good
" ship with the roots. 346.
7. " *Nardis montana tuberosa*.
" Knobbed mountain valerian.
" Discovered in a botanic excursion by J. Tradescant. 388.

any

any plant extant in the known world, that will not, with proper care, thrive in this kingdom.

When his house at South Lambeth (then called Tradescant's Ark) came into Ashmole's possession, he added a noble room to it, and adorned the chimney with his arms, impaling those of Sir William Dugdale (whose daughter was his third wife), where they remain to this day.

This house belongs at present to John Small, Esq; who, about twelve years ago, purchased it of some of Ashmole's descendants; and my house, once a part of Tradescant's, is adjoining thereto.

It were much to be wished, that the lovers of Botany had visited this once famous garden, before, or at least in, the beginning of the present century. But this seems to have been totally neglected till the year 1749, when yourself, and the late Dr. Mitchel, favoured the Royal Society (i) with the only account now extant, of the remains of Tradescant's garden. In it, Sir, you seem to confine the extent thereof to

8. " John Tradescant introduced a new strawberry, with
" very large leaves, from Brussels; but, in the course
" of seven years, could never see one berry completely
" ripe. Page 528.
9. " John Tradescant procured a new and great variety of
" plums from Turkey, and other parts of the world. 575.
10. " The Argier, or Algier apricot. This, with many other
" sorts, John Tradescant brought with him, returning
" from the Argier voyage, whither he went with the
" fleet that was sent against pirates, A° 1620." 579.

Thus far Parkinson; but whether or no these plants bear his name at this period, I can no more pretend to assert, than that all the species therein mentioned are even now existing in our gardens.

(i) Philosophical Transactions, Vol. XLVI. p. 160.

that

that now belonging to Mr. Small's house. I believe it was otherwise; and, on the account of the great number of plants, trees, &c. am inclined to think that Tradescant's garden extended much farther. Bounded on the West by the road, on the East by a deep ditch (still extant) it certainly extended a good way towards the North, and took in not only my orchard and garden, but also those of two or three of my next neighbours; and some ancient mulberry trees, planted in a line towards the North, seem to confirm this conjecture.

When the death of John Tradescant happened, I have not been able to discover, no mention being made thereof in the Register Book of Lambeth Church.

A singular monument (of which I herewith send you a drawing, Tab. IV. and V.) was erected, in the South East part of Lambeth church-yard, in 1662, by Hester, the relict of John Tradescant the son, for himself, and the rest of this family, which is long since extinct (*k*).

This once beautiful monument hath suffered so much by the weather, that no just idea can now, on inspection, be formed of the North and South sides. But this defect is happily supplied from two fine drawings, preserved in Mr. Pepys's Library at Cambridge. We see

On the East side Tradescant's arms.

On the West A hydra, and under it a skull.

(*k*) John the grandson, buried 15th September 1652.

John the son, buried 25th April 1662.

Hester, widow of John Tradescant, buried 6th April 1678.

From the Register of Lambeth Church.

On

On the South { Broken columns, Corinthian capitals, &c. supposed to be ruins in Greece, or some other eastern countries.

On the North { A crocodile, shells, &c. and a view of some Egyptian buildings.

Various figures of trees, &c. in relievo adorn the four corners of this monument.

The following remarkable epitaph (preserved at Oxford, and printed in Mr. Aubrey's *Antiquities of Surrey*, p. 11.), was intended for, but never placed upon, this monument.

Know, stranger, e'er thou pass, beneath this stone
 Lie John Tradescant, grandfire, father, son.
 The last dy'd in his spring; the other two
 Liv'd till they had travelled art and nature thro'.
 As by their choice collections may appear,
 Of what is rare in land, in seas, in air:
 Whilst they (as Homer's Iliad in a nut)
 A world of wonders in one closet shut.
 These famous antiquarians that had been
 Both gardeners to the Rose and Lilly Queen,
 Transplanted now themselves, sleep here; and when
 Angels shall with their trumpets awaken men,
 And fire shall purge the world, these hence shall rise
 And change their garden for a paradise.

Before I conclude, I must beg leave to add a list of the portraits of the Tradescant family, now in the Ashmolean Museum. I cannot, however, conceive why both father and son are therein called Sir John, as it does not appear either of them were ever knighted. But so it is in the Oxford list communi-
 cated

cated to me, some time since, by the late worthy and learned Mr. William Huddesford, keeper of the Ashmolean Museum.

1. Sir John Tradescant, senior. A three quarter piece, ornamented with fruit, flowers, and garden roots.
2. Ditto. After his decease.
3. A small three quarter piece. Water colours.
4. A large piece, of his wife, son, and daughter. Quarter length.
5. Sir John Tradescant, junior, in his garden. Half length, a spade in his hand.
6. Ditto, with his wife, in one piece. Half length.
7. Ditto, with his friend Zythepfa of Lambeth, a collection of shells, &c. upon a table before them. A large quarter piece, inscribed Sir John Tradescant's second wife, and son.

These pictures have no date, nor painter's name, as I can yet find. They are esteemed to be good portraits. Who the person was, called in the picture Zythepfa, I never could learn. He is painted as if entering the room, and Sir John is shaking him by the hand,

I have the honour to remain, with great esteem,

S I R,

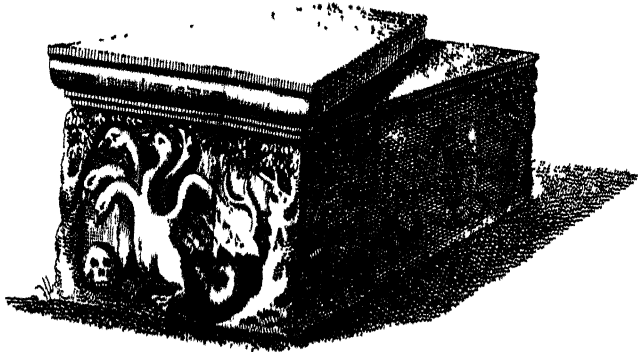
Your most faithful,

humble servant,

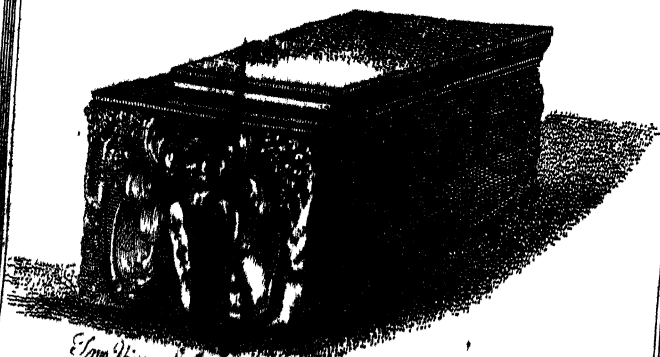
Doctors-Commons,
November 2, 1772.

And. Coltee Ducarcl.

From the West



From the East



Two Views of the Monument of JOHN TRADESCANT in the Church
of St. Mary Lambeth 1773.

As Purchased

Howells

From the SOUTH.

Philos. Trans. Vol. LXIII, Tab. V, p. 33.



In the PEPYSIAN Library.



XIII. *De intenso Frigore Mensibus Januario 1767 ac 1768, & Novembri 1770, observato* Franequeræ, *Auctore* J. H. Van Swinden, *Philos. Profess. in Academia* Franeker. *Societ. Harlemono-Hollandicæ Socio.*

IN Transactionum Philosophicarum, tomis LVIII. & LIX. vidi nuperrime, illustrissimos horum Actorum Collectores observata quædam inseruisse circa acerbum frigus anni 1768: cum vero exinde liqueat hujusmodi annotationes usu non carere, in medium proferam quæ, vel propriis vel alienis observationibus, in patria institutis, ad notitiam meam pervenerunt.

Anno 1767, peritissimus telescopiorum catoptrorum artifex Vander Bild, thermometrum Fahrenheitianum observavit,

Jan. 6°.	h. 8	A. M. ad gr.	. . .	16
	h. 10	P. M.	2
7°.	h. 7½	A. M.	— 2
	12	+ 12
	7	P. M.	5
	9	7
8°.	7½	A. M.	12

Minima altitudo fuit ad minimum — 2, nam mercurius totus intra bulbum erat reconditus, adeoque ad minimum ad 2 gr. infra 0, cum bulbus a 0 hac quantitate distet. Mercurius ab alio hic visus fuit ad — 5, ut mihi memoratus Vander Bild narravit. Si vero comparationis tabellam formemus, habebimus,

Franeq. Amst. Leidæ. Zwaneburgi. Wel.
Die 7° Jan. 2 4 5 6 — 3

Hæ autem observationes omnes, mane hora 7 aut 8 institutæ fuerunt, excepta ea quam in pago Wel, prope Silvam Ducum instituit Rev. Palier: ibi therm. fuit,

h. 7. A. M. ad — 2½	h. 2. ad 7½	h. 9. P. M. — 3
8. . . . — 2	6. 0	10. . . . 0
9. 0	7. — 1	
10. 1	8. 2	

(Videat. Harlem. Societ. Act. Belg. T. IX. p. 649. Partis III.). Notatum autem est maximum frigus A° 1740, in Hollandia fuisse — 2, ac — 4.

A° 1768. Observationes a meipso institutæ sunt. Die Jan. 3, altitudo maxima, h. 12 ad gr. 13, minima gr. 7; h. P. M. 5. vento N. O. die 4, maxima, 20°, minima 12, vento O. ac Z. O. die 5, maxima 23°, minima 18, h. 11. P. M. vento Z. Z. O; nixit hoc die. Die vero 6, hora 8½ A. M. ther. 3½, Vento Z. O. cælo sereno: hora 9, ad 6°, h. 11½ ad 6°, h. 11, ad 12; h. 4. P. M. ad 10, hora 10 ad 11, cum valido vento orientali. Si iterum comparationem instituamus, habebimus,

Franeq.

Franeq.	Amfter.	Zwanenb.	Leidæ.	Wel.
$3\frac{1}{2}$ Die 6.	7 Die 4.	6 Die 3. 7 Die 4. 8 6.	4 Die 4. 8 Die 6.	$-3\frac{1}{2}$ Die 3. -1 Die 4. $+7$ Die 6.

Aliis in locis, uti Lipsiæ, Coloniae, Regiomonti, &c. mercurium multum infra zero descendisse novellæ publicæ retulerunt. Sane si ad frigoris intensitatem & durationem adtendamus, hanc hiemem inter acerbissimas referemus: sed hæc hæctenus, tantum enim maximos frigoris gradus adnotare volui.

Verum quæ mense Novembri 1770, hic observavi rariora sunt.

Die Nov. 17, gelu cum nive incepit, vento O. Die 18, h. 8, therm. $27\frac{1}{2}$. Die 19, h. 8. A. M. 24° , hora 2, P. M. 28; h. 5, 19° : h. 10, 16° . vento O, ac O. Z. O. At die 20, h. $7\frac{1}{2}$. therm. 9° . vento Z. Z. O. dein vero celeriter adscendit: nam hora 2, erat jam ad 25° . vento O. Z. O. cœlo sereno: h. 6, ad 32° , vento Z. Z. W. cum nive, hora 10, ad 32° . die 21, h. 10, ad 36° . Nescio an nostris in regionibus, tanti frigoris hoc mense observati dentur exempla. An imminens nix ejus causa exstitit? Si vero comparationem cum aliis locis instituamus, habebimus,

Franeq.	Amst.	Zwanenb.	Leidæ.	Hag.	Berol.
9	22	25	17	21	$17\frac{1}{2}$
Die 17, 23. h. 11, P. M. 36.					

Maxima mutatio hic fuit 27° . Magnam autem inter loca, haud multum ab invicem distita, differentiam dari liquet, & sæpe fuit observatum: omnis

tamen a sola thermometrorum situ vario repeti nequit.

Ad hanc frigoris intensitatem accedit, celeremque hanc temperiei mutationem multum superat, quod in vico Tooting, die 22 Nov. 1748, observavit Rev. Miles (Philos. Transact. N° 491. T. XLVI. p. 3.), sc. Die 21 Nov. fuit therm. ad 7° gr. infra 0, seu congelationis punctum. Die 22, h. $4\frac{1}{2}$, A. M. $14\frac{1}{2}$ infra 0. h. 7, gr. 5 infra 0, vento W. ac Z. W. Mane, hora 10, jam pluebat thermom. indicante 5 supra 0, hora $8\frac{1}{2}$, P. M. therm. erat ad 12 supra 0. Unde mutatio fuit gr. $26\frac{1}{2}$. Ait vero Cel. Maty (Jour. Britan. T. V. p. 124.), hoc therm. cum Fahrenheitiano convenire, nisi quod punctum 0 in puncto congelationis fuerit notatum. Unde in scala Fahrenheitiana erunt frigoris gradus hi.

22 Nov. 1748, h. $4\frac{1}{2}$ gr. $17\frac{1}{2}$. h. 7. gr. 27. h. 10, gr. 37. cum pluvia, h. $8\frac{1}{2}$, P. M. gr. 42. Frigus ergo minus eo quod hic observavi.

Franeq. 13 Nov. 1772.

J. H. Van Swinden.

P. S. Anno 1771, die Jan. 13, vidi therm. ad 8 gr. die Feb. 11, ad gr. 13. die Jan. 19, 1772, ad 11° . Verum de his alia opportunitate latius.

XIV. *An Inquiry into the Quantity and Direction of the proper Motion of Arcturus ; with some Remarks on the Diminution of the Obliquity of the Ecliptic : By Thomas Hornsby, M. A. Savilian Professor of Astronomy in the University of Oxford, and F. R. S.*

Read Dec. 24,
1772.

AS an accurate knowledge of the position of the fixed stars is of the greatest importance, being the basis and foundation of astronomy, it is no wonder that the astronomers of different nations have given great attention to this matter. By comparing *antient* with the best *modern observations*, it appears that some of the fixed stars have a *proper motion*, independent of any motion hitherto known in our own system ; or that, in other words, the *angular distances of the fixed stars have not always continued the same*, and in some of them the alteration is so very considerable as to be easily perceived in the course of *a few years*, with instruments accurately made, and nicely adjusted. Of all the stars visible in our hemisphere, the *variation in the place of Arcturus is the most remarkable*, and such as cannot possibly be attributed to the uncertainty of observation. It has accordingly been taken notice

tice of by many astronomers : in particular, Dr. *Halley* mentions it in N° 355 of the Philosophical Transactions : Mr. *Cassini*, in the Memoirs of the Academy of Sciences for 1738, p. 231, has shewn, that there is a variation of *five minutes* in the latitude of that star between his own time and that of Tycho, in an interval of a *century and a half* : and M. le *Monnier*, in the Memoirs of the Academy of Sciences for 1767, p. 417, proves, that the *latitude* of Arcturus varies at the rate of *two seconds* every year ; and that the *longitude* decreases at the rate of 60'' in a hundred years *. But as an *inquiry* as well into the true *quantity* as into the *direction* of this motion has not hitherto been made public, I propose to give some account of my own observations made expressly with this view in the years 1767 and 1768, with a transit instrument of 44 inches, and a moveable mural quadrant of 33 inches, both constructed by Mr. Bird, and of the conclusions resulting from a comparison between them and some observations made by Mr. Flamsteed in 1690.

It may perhaps be objected, that the differences of right ascension, as determined by Mr. Flamsteed's mural instrument, are not to be depended upon from the very nature of his instrument. Mr. Flamsteed was himself too good an observer not to be aware of this ; and accordingly in the Prolegomena to the third volume of the *Historia Cælestis*, p. 132, he informs us in what manner he determined the error of

* See also the Memoirs of the Academy of Sciences for 1769, p. 21. See also *Astronomiæ Fundamenta*, by the Abbé de la Caille; who, in reducing his observations of Arcturus, supposes the annual motion of declination in that star = 19'',0. p. 169, and 187.

the plane at different distances from the zenith. By distributing these errors in the best manner I could, I am of opinion, that the error of the plane of his instrument may be supposed to decrease uniformly at the rate of half a second in time for every degree of zenith distance from 28° to 60° , the error being $39''$ at the former, and $23''$ at the latter, by which quantity stars passed the horary wire, in his instrument, before they came to the true meridian. It should seem also that the error continued nearly the same from 60 to 75 degrees, being at the latter only $22''$: but that it decreased irregularly from 75° to 85° , viz. $1''$ in time for each degree from 75° to 80° , and $0'',4$ for each degree from 80° to 85 degrees. The mural arc was fixed upon a stone pier, the southern part of which was found to settle yearly, from whence the error of the line of collimation to the south necessarily became every successive year greater and greater. As Mr. Flamsteed seems not to have had any method of adjusting his instrument by a plumb-line, these errors must have been irregular at different seasons of the same year, and were perhaps never truly determined. But as the observations here referred to were made on the same day, and within the compass of an hour, they are probably not affected with this latter error. We are at present concerned with the difference of two zenith distances, and not with the absolute quantity of those zenith distances. The conclusions may indeed be affected with an error in the divisions; and from the examination which I have been able to make, I am of opinion that the arc of Mr. Flamsteed's instrument was not of the proper quantity; and that, though the observations

generally

generally erred in defect, they in some parts erred in excess.

On the 14th of February, 1690, Mr. Flamsteed observed, that a small star, of the seventh or eighth magnitude, whose place is not determined in the British catalogue, and which star was named by him *Infra Arcturum*, preceded Arcturus three seconds in time, or $3'',3$, when an allowance is made for the error of the plane of the instrument $= 0' 42'', 6$, and was $26' 30''$ to the south of Arcturus *. By a mean of eight observations made at Oxford, on and near June the 10th, 1767, with the transit instrument, and with a refracting telescope of eight feet, furnished with a micrometer; the difference of right ascension was $1', 8'', 75$ of a degree, the star following Arcturus; and by a mean of three observations, the extremes differing only $3''$, the small star was $23' 55'', 0$ to the south of Arcturus.

The right ascension of Arcturus and the small star being nearly the same, the change in declination ought to be so likewise. But, from the observed difference in declination, the right ascension of the two stars must vary unequally, though with a very small difference. Accordingly it appears from computation (in which the annual precession is supposed $= 50'', 35$, the obliquity of the ecliptic at the middle of the interval of the time $= 23^\circ 28' 30''$, and the right ascensions and declinations of the two stars taken at a mean between the times of observation) that the variation of Arcturus in right ascension was $3270'', 6$, and of the small star $3277'', 6$ in

* This is the only observation of that star made by Mr. Flamsteed.

77,287 years. Therefore the right ascension of Arcturus alters less than that of the star; and consequently Arcturus should in 1767 have followed the star by $42'',6$. But the star was observed to follow Arcturus by $1\ 8',75$. The right ascension therefore of Arcturus has increased less than that of the star, or Arcturus has moved westward $1\ 51'',35$ in 77,287 years; and has gone southward $2\ 35''$ in the same time, supposing the small star not to have moved, which is highly probable.

On the same day the difference of right ascension in time between the star η Bootis and Arcturus was $21\ 32''$ of mean solar time, $= 5^{\circ}\ 24'\ 02'',2$, when a proper allowance is made for the going of the clock, and for the error of the plane of the instrument; and the difference of declination was $50'\ 45'',6$, when an allowance is made for refraction. On the 24th, 26th, and 29th of May, and the 9th of June, of the year 1768, I determined the difference in right ascension to be $21\ 27''$ of sidereal time by the two former observations, and $21\ 26\frac{1}{4}''$ by the two latter, the difference in declination being $49'\ 48'',7$, by a mean of the observations in May, the extremes differing only four seconds. It appears from computation, that between the times of observation the variation of η Bootis in right ascension was $3371'',7$, and $1417'',3$, in declination; of Arcturus $3311'',7$ in right ascension, and $1347'',9$ in declination: The difference of variation in right ascension is $1\ 0''$, and of declination $1\ 9'',4$; by the former the difference in right ascension was diminished, and in declination increased by the latter, agreeably to the places of the two stars. The difference in right ascension

therefore in 1768, if neither of the stars had moved, should have been $= 5^{\circ} 23' 02'', 2$, and $51' 55''$ in declination; but they were observed to be $5^{\circ} 21' 43'', 4$, and $49' 48'', 7$. Arcturus therefore by this observation has in 78,257 years gone $1' 18'', 8$ to the west, and $2' 06'', 3$ to the south, supposing η Bootis not to have any proper motion.

On the 5th of April, 1691, the difference in right ascension between η Bootis and Arcturus was $21' 33''$ of mean solar time, $= 5^{\circ} 24' 14'', 0$; and the difference of declination $50' 45'', 6$, as in the preceding example. The difference of variation in right ascension is $59'', 1$, and in declination $1' 8', 4$. The difference of right ascension therefore at the latter end of May, 1768, should have been $5^{\circ} 23' 14'', 9$, and $51' 54'', 0$ in declination; but, according to observation, they were $5^{\circ} 21' 43'', 4$, and $49' 48'', 7$. Arcturus therefore, according to this observation, has moved $1' 31'', 5$ to the west, and $2' 05'', 3$ to the south in 77,120 years.

On the 4th of May, 1691, the difference of right ascension between η Bootis and Arcturus was $21' 33''$ of mean solar time, $= 5^{\circ} 24' 14'', 3$ when allowance is made for the going of the clock and the error of the plane of the instrument, and the difference of declination on the 3d of May $= 50' 50'', 6$. According to computation, those differences should have been $5^{\circ} 23' 15'', 2$ and $51' 59'', 0$ respectively; but they were observed to be $5^{\circ} 21' 43'', 4$, and $49' 48'', 7$. Arcturus therefore in 77,071 years has moved $1' 31'', 8$ westward, and $2' 10'', 3$ southward. N. B. The zenith-distance of Arcturus,

as determined by Mr. Flamsteed, on the 4th of May, is manifestly erroneous.

On the 27th of May, 1692, γ Bootis preceded Arcturus in right ascension by $21' 32'',5$ of mean solar time, $= 5^{\circ} 24' 10'',1$, the difference of declination being $50' 50'',6$. In 75,978 years the difference of right ascension should have been $5^{\circ} 23' 11'',8$ and $51' 58'',0$ in declination; but those differences were observed to be $5^{\circ} 21' 43'',4$ and $49' 48'',7$. Arcturus therefore has moved $1' 28'',4$ westward, and $2' 09'',3$ southward.

On the 27th of May, 1692, Arcturus preceded π Bootis in right ascension by $24' 35'',5$ of mean solar time, $= 6^{\circ} 9' 32'',2$, when an allowance is made for the going of the clock and the error of the plane of the instrument, the difference of declination being $3^{\circ} 2' 28'',9$. On the 24th and 26th of May, and 5th of June, 1768, the difference of right ascension between the same stars observed at Oxford was $24' 44'',58$ of sidereal time, $= 6^{\circ} 11' 9'',1$, the difference of declination being $2^{\circ} 58' 24'',2$. In 1768, the difference of right ascension should have been $2'',7$ greater, $= 6^{\circ} 9' 34'',9$; and the difference of declination $1' 31'',7$ less, $= 3^{\circ} 0' 57'',2$. But they were observed to be $6^{\circ} 11' 9'',1$, and $2^{\circ} 58' 24'',2$. Arcturus therefore in 75,978 years has, by a comparison with this star, moved $1' 34'',2$ westward, and $2' 33'',0$ southward.

Again, the difference of declination between Arcturus and π Bootis was observed to be $3^{\circ} 2' 33'',9$ on the 14th of February, 1690, when the difference of right ascension between these two stars was not observed by Mr. Flamsteed. It appears by compu-

tation, that the difference of variation in declination between the times of observation was $1^{\circ} 34'',5$, by which quantity the difference of declination was diminished, and should therefore in 1768 have been $3^{\circ} 0' 59'',4$. But it was $2^{\circ} 58' 24'',2$ by actual observation. Arcturus therefore by this observation has moved southward $2' 35'',2$ in 78,255 years.

By the foregoing comparisons Arcturus appears to have moved as in the following table.

	years.	Westwd.	Southwd.
		'	'
By the small star Feb. 14, 1690, in	77,237	1 51,35	2 35,0
" Bootis Feb. 14, 1690, in	78,257	1 18,8	2 6,3
" Bootis Apr. 5, 1691, in	77,120	1 31,5	2 5,3
" Bootis May 4, 1691, in	77,071	1 31,8	2 10,3
" Bootis May 27, 1692, in	75,978	1 28,4	2 9,3
By " Bootis May 27, 1692, in	75,978	1 34,2	2 33,0
" Bootis Feb. 14, 1690, in	78,257	not obs.	2 35,2

As the quantity of the motion of Arcturus southward in declination, as deduced from a comparison with η Bootis, differs considerably from the quantities given by the small star and π Bootis, which agree very nearly together, I have compared η Bootis with some of the neighbouring stars, as that star, though of the third magnitude only, may have a small motion of its own.

On the 14th of February, 1690, the difference of declination between η and π Bootis was observed by Mr. Flamsteed to be $= 2^{\circ} 1' 47'',8$. By computation, that difference in 1768 should have been $2' 43'',9$ less, $= 2^{\circ} 9' 3'',9$: but it was actually observed

observed to be $2^{\circ} 8' 34'',3$ only. The star η Bootis, therefore appears by this comparison to have moved southward $29',6$ in 78,257 years.

On the 27th of May, 1692, η Bootis was observed by Mr. Flamsteed to be $2^{\circ} 11' 37'',8$ to the north of π Bootis, which quantity should by computation be $2' 39'',1$ less in 1768, or $2^{\circ} 8' 58'',7$. But it was found to be $2^{\circ} 8' 34'',3$. The star η therefore appears to have moved southward $0' 24'',4$ in 75,978 years.

On the 25th of April, 1693, η Bootis was observed to be $40' 20'',8$ to the south of f Bootis, a star of the 6th magnitude; and by myself that difference was observed to be $42' 37'',5$, by taking the mean of two observations on the 24th and 26th of May, 1768, differing only $4'',7$. According to computation, the variation of η Bootis in declination during the interval of the two observations was $1359'',3$, and of f Bootis $1256'',0$; and therefore the difference of variation in declination was $1' 43'',3$, by which the distance of the stars was increased. The difference in declination therefore in 1768, if neither of the stars moved, should have been $0^{\circ} 42' 04'',1$; but it was observed to be $33'',4$ greater, by which quantity therefore η Bootis must have moved southward in 75,052 years.

By reducing all the foregoing deductions to 78 years, Arcturus appears to have moved,

Westw. Southw.

By the small Star, Feb. 14, 1690	1' 52,380	2 36,43
η Bootis Feb. 14, 1690	1 18,541	2 5,88
η Bootis Apr. 5, 1691	1 32,557	2 6,75
η Bootis May 4, 1691	1 32,906	2 11,87
η Bootis May 27, 1692	1 30,752	2 12,74
By π Bootis May 27, 1692	1 36,707	2 37,07
π Bootis Feb. 14, 1690	not observ.	2 34,69

But the Star η Bootis appears also to have moved Southward.

By π Bootis Feb. 14, 1690	0 29,503
π Bootis May 27, 1692	0 25,049
By f Bootis Apr. 5, 1693	0 34,712

By a mean 0 29,755

As Arcturus appears to have moved southward of η Bootis 2' 9'',31, by taking a mean of the four quantities resulting from the comparisons with that star; and as η Bootis has also moved southward of some of the neighbouring small stars by 29'',755 in the same time, Arcturus upon the whole has moved 2' 39''06 to the south, by the comparisons with η Bootis only; and therefore, by taking a mean of all the results, Arcturus has altered its right ascension less than the neighbouring stars by 1' 33'',97 in 78 years, in which time it has also moved 2' 36'',81 to the south of the same stars.

In order to see how far the motion of right ascension is to be depended upon, which is deduced from the above comparisons, I have selected and computed the following observations, made at Shirburn castle

castle with a transit instrument of $5\frac{1}{2}$ feet, placed exactly in the plane of the meridian, and consequently more to be relied upon than those made with a mural instrument.

By a mean of five observations, made on the 7th, 12th, 23d, 24th, and 31st of May, 1741, O. S. the difference in right ascension between η Bootis and Arcturus was $5^{\circ} 22' 38''.9$, the extremes differing only $4''.4$ of a degree. The difference in the variation of right ascension to the end of May, 1768, is $20''.5$, by which the ascensional difference is diminished. It should therefore have been $5^{\circ} 22' 18''.4$; but it was observed to be $5^{\circ} 21' 43''.4$. Therefore in 27 years Arcturus has moved westward $35''.0$.

On the 16th and 20th of May, 1744, the difference in right ascension between η Bootis and Arcturus was $5^{\circ} 22' 30''.0$ by each of the observations, which difference should have been, supposing neither of the stars to have any proper motion, $5^{\circ} 22' 11''.7$ in May, 1768. But it was found to be $28''.3$ less; by so much therefore had Arcturus moved westward in 24 years.

On the 24th of May, and 8th of June, 1746, the difference in right ascension between the same stars was $5^{\circ} 22' 26''.2$, by taking a mean of the two observations; that difference should have been $5^{\circ} 22' 09''.5$ in 1768. But it was observed $= 5^{\circ} 21' 43''.4$. Arcturus therefore in 22 years has moved $26''.1$ to the west.

Lastly; on the 16th of April, and 27th and 28th of May, 1747, the difference in right ascension between η Bootis and Arcturus, by taking a mean of the three observations, was $5^{\circ} 22' 25''.0$. By computation

putation the variation in the difference of right ascension was $16'',0$, by which the ascensional difference should have been diminished, and $= 5^{\circ} 22' 09'',0$. But by observation it was found $= 5^{\circ} 21' 43'',4$: Arcturus therefore by this last observation appears to have gone $25,6''$ westward.

By the observations therefore at Shirburn Castle Arcturus appears to have gone westward as in the following table; in the last column of which are contained the quantities resulting from the observations of each year, reduced to 78 years.

	''	'	''
1741	0 35,0	1	41,11
1744	0 28,3	1	31,97
1746	0 26,1	1	32,59
1747	0 25,6	1	34,90
	Mean	1	35,14

The mean of all the observations, when reduced to an interval of 78 years, is $1' 35'',14$, which differs only $1'',17$ from the mean of the other comparisons.

As then the proper motions of Arcturus westward in right ascension $= 1' 33'',974$, and $2' 36'',81$ in declination southward, seem well established, the real motion of Arcturus is inclined in an angle of $30^{\circ} 56'$ to the west of the meridian or horary circle, and to be in that direction $3' 2'',81$ in 78 years, or at the rate of $2'',343$ in a year. As this direction of its motion is nearly perpendicular to the plane of the ecliptic, the latitude of Arcturus must diminish yearly almost in the same proportion; and its longitude will alter less than that of other stars, though not so considerably as its right ascension.

The

The proper motion of Arcturus then, in right ascension Westward, being $1'',205$, and in declination $2'',005$, its annual precession in right ascension is $41'',108$, and in declination $19'',133$; and the true right ascension of Arcturus, on Jan. 1, 1773, is $211^{\circ} 19' 47'',4$, and declination North $20^{\circ} 22' 23'',3$.

As none of the other principal stars have been found to have a motion so considerable as this, though many of the stars of the first magnitude, as for instance, Sirius, Procyon, α Aquilæ, α Orionis, as also β Aquilæ of inferior magnitude, do really vary their positions (and perhaps all of the first order will hereafter be found to have a proper motion), we may, I think, fairly conclude that Arcturus is the nearest star to our system, visible in this hemisphere. If therefore the annual parallax of the fixed stars can ever be discovered, that is, if the diameter of the annual orbit bear a sensible proportion to the distance of the nearest fixed star, it is most likely to be discovered from the observations of Arcturus. The system of the world, considered in an enlarged sense, and agreeable to the idea we may entertain of an all-powerful benevolent Creator, may be taken to occupy the whole abyss of space, and to consist of an assemblage of bodies, having different magnitudes, and emitting various degrees and modifications of light. The apparent change of situation visible from the planet which we inhabit, and which revolves round one of the great bodies constituting a part of the general system, as a center, may be owing either to the motion of our own system in absolute space, or, if our system should be at rest, to a real motion in

the stars themselves: from whence the angular distances of the stars must vary in proportion to the velocity of those motions, or to the direction of those motions with respect to ourselves. I have reason, at present, to believe that a small motion may be discovered in the star α Ceti, and perhaps in other stars that vary in degrees of brightness, which the diligence of future astronomers will discover, and perhaps in less time than at first sight might seem necessary, when we consider the several improvements which have of late been made in the methods of observing the heavenly bodies.

As the motion of Arcturus in declination (the quantity of which we have thus endeavoured to ascertain) has been often acknowledged, it is matter of wonder that some astronomers, by comparing either the altitude or zenith distance of the Sun's limb with Arcturus, without previously settling the quantity of that star's motion in declination, or at least doing it indirectly, should endeavour to determine whether the obliquity of the ecliptic hath remained constant, or still continues to diminish, as it should seem to have done for many centuries past, from the observations of successive astronomers. Mr. Cassini, and Mr. le Monnier, have both practised this method, and are of opinion, that the obliquity of the ecliptic hath not altered; or, if it hath altered, that the quantity of its alteration is not near so considerable as hath been imagined by some celebrated astronomers. By observing for several days before and after the solstice the altitude or zenith distance of the Sun's limb, and that of a star situated near the same parallel, the differences to be remarked in process of time

time in the distances of the Sun from that star (the motion of the star in declination being allowed for during that interval of time), will be the quantity by which the Sun will have approached to or have receded from the star. If the star were absolutely a fixed point, and the observations sufficiently numerous, that, by taking a mean, the necessary and unavoidable errors in observation might either be considerably diminished or almost annihilated, the method might be practised to great advantage. But as the star (Arcturus) had a proper motion, and its apparent place was continually varying from the effect of the nutation of the earth's axis; as the limb of the Sun was sometimes approaching to, and sometimes receding from, the star, by a kind of libratory motion from the effect of the nutation; also and as the obliquity of the ecliptic itself was, in all probability, continually diminishing; from a combination and as it were involution of these motions no certain conclusion could be drawn, since, in the space of a few years, the apparent obliquity may be the same, and yet the mean obliquity may have diminished, or perhaps, in the space of a few years, the obliquity may appear to have increased, when it may really have become less. Whereas, by reducing the observations to their mean position, and by assigning to each known cause its proper and allowed effect, a regularity and uniformity must necessarily take place, as far at least as is consistent with the unavoidable errors in observing.

Mr. Cassini, in the Memoirs of the Academy of Sciences for 1767, acquaints us, that, in 1748, the apparent distance of Arcturus from the upper limb

of the Sun, at the time of the solstice, was the same as in 1766.

In 1748. Distance of Arcturus from	} 0 1 11 111
the solstitial limb of the Sun	} 3 13 36 40
Altitude of Arcturus	61 41 17 0
Therefore the apparent solstitial	} 64 55 13 40
altitude	
In 1766. Distance of Arcturus from	} 3 19 32 0
the solstitial limb of the Sun	
Altitude of Arcturus	61 35 42 0
Therefore the apparent solstitial	} 64 55 14 0
altitude	

The same astronomer has, in the Memoirs for 1759, p. 325, communicated the following conclusions.

	Diff. of the star from ☉'s limb.	Reduction.	Solstitial distance.
1763.	0 1 11	1 11	0 1 11
June 14.	3 7 29	+ 11 1	3 18 30
15.	3 10 16	+ 8 13	3 18 29
25.	3 15 40	+ 2 48	3 18 28
July 1.	2 59 1	+ 19 22	3 18 23
2.	2 54 55	+ 23 33	3 18 28
3.	2 50 18	+ 28 8	3 18 26
		Mean	3 18 27

Mr. le Monnier, in the Memoirs for 1762, p. 269, has published the following distances of Arcturus from the limb of the Sun, reduced to the solstitial point, with a view to obtain differences in the apparent obliquity of the ecliptic: and, from the obser-

ventions

vations made with the gnomon of St. Sulpice, and communicated by Mr. de Monnier, in the same volume, it should seem that that astronomer is of opinion, that the obliquity of the ecliptic hath no other variation than what the nutation of the earth's axis will occasion; and that therefore we must either abandon the absolute diminution of the ecliptic, or at least suppose it extremely small, since, in the space of eighteen years, it hath not produced a sensible alteration.

1738. $\begin{smallmatrix} 0 \\ 3 \end{smallmatrix} \begin{smallmatrix} 10 \\ 10 \end{smallmatrix} \begin{smallmatrix} 15 \\ 15 \end{smallmatrix}$

1740. $\begin{smallmatrix} 3 \\ 3 \end{smallmatrix} \begin{smallmatrix} 11 \\ 11 \end{smallmatrix} \begin{smallmatrix} 5 \\ 5 \end{smallmatrix}$

1742. $\begin{smallmatrix} 3 \\ 3 \end{smallmatrix} \begin{smallmatrix} 11 \\ 11 \end{smallmatrix} \begin{smallmatrix} 48 \\ 48 \end{smallmatrix}$

1763. $\begin{smallmatrix} 3 \\ 3 \end{smallmatrix} \begin{smallmatrix} 18 \\ 18 \end{smallmatrix} \begin{smallmatrix} 40 \\ 35 \end{smallmatrix}$ with the mural quadrant of 5 feet.
 $\begin{smallmatrix} 3 \\ 3 \end{smallmatrix} \begin{smallmatrix} 18 \\ 18 \end{smallmatrix} \begin{smallmatrix} 35 \\ 35 \end{smallmatrix}$ with the large mural instrument.

As the result of the observations only, and not the observations themselves, are communicated, I have only to observe, that there is a very considerable difference between the conclusions of the two astronomers for the same year 1763, and, at the same time, to declare my suspicion, that if the *apparent* (for such I apprehend them to be) were reduced to the *mean* distances, they would probably afford a confirmation of the diminution of the ecliptic. For the following observations of the Sun's zenith distance, made at Shisburn Castle, near the summer solstices of the years 1743, 1746, 1748, and 1766, and of Arcturus in the years 1743, 1746, and 1766, when reduced to their mean state at the solstice, do not confirm the assertion of Mr. Cassini, but are an evident
and

and absolute proof that the obliquity of the ecliptic has sensibly diminished during an interval of 23, and even of 18 years.

The observations of 1743 were made with a mural quadrant of five French feet, constructed by the late Mr. Sisson: but as the linear divisions were found to be somewhat less accurate than was expected, and as the body of the quadrant was not framed with proper strength and solidity, Mr. Bird was employed in the summer of the year 1745, by the Earl of Macclesfield, (the body of the instrument having been strengthened by screwing a large and broad plate of brass upon the cross-bars), to put a set of points upon the limb between the 90 and 96 arches of linear divisions. By these operations the line of collimation was found to have varied, and to be $= 6', 3$, by which the zenith distances were given too small, by the positive divisions, from the end of 1746 to the end of June 1751, when Mr. Bird bisected the spaces between the points which he had formerly added in 1745. But after the year 1751, the error of the line of collimation was $= 2'', 6$, as appears from observations of γ Persei, β and γ Draconis, by which the zenith distances are also given too small; and in that state the instrument continued to the year 1767, when a new set of wires was put into the telescope, and the line of collimation thereby altered. The error of the line of collimation from 1743 to 1745 cannot directly be ascertained, for want of zenith observations; but, from some indirect methods, it should seem that the error was as nearly as possible $= 2''$, to be added to the observed zenith distances.

[III]

	Observed zenith distance.	Barometer.	Thermo- meter.	Refrac- tion.	Sun's semi- diameter.	Dist. from solstice.	Observation reduced to solstice.
1743.							
June	7. Up. L. 28 37 39	29,54	62	30.2	+ 15 48,1	43 5,7	28 10 51,6
	8. Lo. L. 29 3 37	29,36	60	30,8	15 48	57 22,3	28 10 57,5
	9. Up. L. 28 26 44,2	29,46	59½	30	+ 15 47,9	32 3,3	28 10 58,8
	12. Up. L. 28 13 14,5	29,72	61½	29,9	+ 15 47,6	18 31,6	28 11 0,4
	18. Up. L. 27 57 9	29,81	67	29,4	+ 15 47,3	2 30	28 10 55,7
	19. Lo. L. 28 27 28,5	29,54	67½	29,8	- 15 47,2	1 16,1	28 10 55
	21. Up. L. 27 54 41,8	29,66	66	29,1	+ 15 47,1	0 2,8	28 10 55,2
	22. Lo. L. 28 26 22,2	29,50	63	29,8	- 15 47	0 3,4	28 11 1,6
	23. Up. L. 27 55 10,2	29,59	61	29,5	+ 15 47	0 28,6	28 10 58,1
	24. Lo. L. 28 27 35,5	29,59	58	30,4	- 15 46,9	1 18,8	28 11 0,2
	25. Up. L. 27 57 15,5	29,39	58	29,5	+ 15 46,9	2 33,7	28 10 53,2
	27. Lo. L. 28 32 34	29,57	58	30,5	- 15 46,9	6 17,9	28 10 59,7

Mean . . . 28 10 58,2
Sun's parallax — 4,1

Nutation 28 10 54,1
+ 6,7

Line of collimation 28 11 0,8
+ 2

Mean solstitial zenith distance, 1743, 11 2,8

1746.							
May	31. Lo. L. 29 57 50,5	29,48	78	30,5	- 15 48,8	1 31 41,9	28 10 50,3
June	5. Up. L. 28 48 24,8	29,62½	64	30,4	+ 15 48,3	53 50	28 10 53,5
	6. Lo. L. 29 13 36,3	29,62	65	30,8	- 15 48,2	47 26,3	28 10 52,6
	7. Up. L. 28 35 58,9	29,53	71	29,4	+ 15 48,1	41 24,3	28 10 52,1
	10. Lo. L. 28 52 1,8	29,28	67	29,8	- 15 47,8	25 49,8	28 10 54
	16. Up. L. 28 0 14,4	28,69	59½	28,7	+ 15 47,3	5 34,6	28 10 55,8
	19. Lo. L. 28 27 5,1	29,60	58½	30,3	- 15 47,2	0 59,8	28 10 48,4
	Up. L. 27 54 45	29,75	63½	29,4	+ 15 47	0 8	28 10 53,4
	Lo. L. 28 26 47,4	29,81	64	30,2	- 15 47	0 40,4	28 10 50,2
	Up. L. 27 59 23,4	29,63	69	29	+ 15 46,9	4 45,9	28 10 53,4
	27. Lo. L. 23 33 0,6	29,63	74½	29,2	- 15 46,9	6 57,1	28 10 51,8
	28. Up. L. 28 4 11,8	29,50	65½	29,3	+ 15 46,9	9 32,9	28 10 55,1
	30. Lo. L. 28 42 5,4	29,22	63½	30,2	- 15 46,9	15 58,	28 10 51
July	4. Up. L. 28 28 16,6	29,67	69	29,6	+ 15 46,9	33 40,1	28 10 53

Mean . . . 28 10 52,5
Sun's parallax — 4,1

Nutation 28 10 48,4
+ 9,4

Error of the line of collimation . . 28 10 57,8
+ 6,3

Mean solstitial zenith distance, 1746, 28 11 4,1

	Observed zenith distance	Baro- meter.	Thermo- meter.	Refrac- tion.	Sun's semi- diameter.	Diff from folstice.	Obs. variations re- duced to the line.
1748.	° ' "			" +	' "	° ' "	° ' "
June 15.	Up. L. 23 1 21.1	29.51½	64	29.3	+ 15 47.4	0 6 38.6	28 10 52.2
16.	Lo. L. 23 50 41.3	29.55	60	50.3	- 15 47.4	4 31.9	28 10 52.3
20.	Up. L. 27 51 45.7	29.86½	63½	29.5	+ 15 47.2	0 5.7	28 10 56.7
21.	Lo. L. 28 20 11.3	29.65½	75 +	29.2	- 15 47.1	0 1.3	28 10 52.1
22.	Up. L. 27 55 3.5	29.67	81½	28.2	+ 15 47	0 21.4	28 10 57.3
23.	Lo. L. 28 27 18.7	29.60½	72 -	29.3	- 15 47	1 6.7	28 10 54.3
24.	Up. L. 27 56 59.7	29.554	65	29	+ 15 46.9	2 16.7	28 10 58.9
29.	Lo. L. 28 40 29.5	29.90	64	30.5	- 13 46.9	14 18.7	28 10 55.2
Mean							28 10 55.8
Sun's parallax							- 4.1
Nutation							28 10 51.7 + 6.1
Error of the line of collimation . .							28 10 57.8 + 6.1
Mean foliutal zenith distance, 1748,							28 11 4.1

1766.	° ' "			"	' "	° ' "	° ' "
June 11.	Lo. L. 28 47 12	29.51	62 -	30.4	- 15 47.7	0 20 42.3	28 11 12.4
12.	Up. L. 28 11 38.3	29.38	61 -	29.6	+ 15 47.0	16 47.1	28 11 8.4
17.	Lo. L. 28 29 47.9	29.15½	66	29.1	- 15 47.3	3 20.2	28 11 9.3
21.	Up. L. 27 54 57.4	29.72	60½	29	- 15 47.1	0 29	28 11 13.5
22.	Lo. L. 28 26 39.6	29.71	70½	29.7	- 15 47	0 11.9	28 11 10.4
23.	Lo. L. 28 27 19.2	29.75	71½	29.5	- 15 47	0 48.0	28 11 13.1
24.	Up. L. 27 50 45.7	29.80	74½	29.4	+ 15 46.6	1 40.1	28 11 10.1
25.	Lo. L. 28 27 41.2	29.19	74	29.2	- 15 46.1	3 10.2	28 11 7.3
27.	Up. L. 28 2 16.5	29.40	67	28.9	+ 15 46.9	7 21.0	28 11 11.7
29.	Lo. L. 28 29 50.6	29.28	66	29.7	- 15 46.9	13 1	28 11 6.4
Mean							28 11 10.5
Sun's parallax							- 4.1
Nutation							28 11 6.1 + 7.6
Error of the line of collimation . .							28 11 14 + 2.6
Mean foliutal zenith distance, 1766,							28 11 16.6

		Observed zenith d. of Arcturus.	Reduc- tion.	Aberra- tion.	Refrac- tion.	Refrac- tion.	Observed zenith distance.
		° ' "	"	"	"	"	° ' "
1743.							
May	12.	31 6 57,8	31	-1,9	+1,4	-2,1	31 7 23,4
	17.	31 6 57,8	34	-0,8	+1,4	+1,8	31 7 34,2
	18.	31 6 57,8	34	-0,6	+1,4	+1,8	31 7 34,4
June	5.	31 6 52,3	34	+2,9	+1,2	+0,9	31 7 31,3
	9.	31 6 50,3	34	+3,8	+1,2	+0,7	31 7 30
	10.	31 6 52	34	+4	+1,2	+0,6	31 7 31,8
	16.	31 6 57	34	+5,3	+1,2	+0,3	31 7 37,8
	18.	31 6 54,8	34	+5,5	+1,2	+0,1	31 7 35,6
	25.	31 6 52	34	+6,7	+1,2	-0,2	31 7 33,7
July	1.	31 6 52	34	+7,8	+1,2	-0,5	31 7 34,5
Mean							31 7 33,6
Error of the line of collimation							+ 2
Mean zenith distance of Arcturus, June 21, 1743,							31 7 35,6
Mean zenith distance of the Sun's center, June 21, 1743,							28 11 2,8
Mean distance of Arcturus from the Sun's center, 1743,							2 56 32,8
1746.							
June	4.	31 7 51,1	34	+2,5	-5,8	+1,1	31 8 22,9
	21.	31 7 53,1	34	+6,1	-5,9	0	31 8 27,3
	22.	31 7 50,2	34	+6,3	-5,9	0	31 8 24,6
	23.	31 7 52,3	34	+6,4	-5,9	-0,1	31 8 27,7
Oct.	9.	31 7 58,6	34	+8,9	-6,4	-5,7	31 8 29,4
Mean							31 8 26,4
Error of the line of collimation							+ 6,3
Mean zenith distance of Arcturus, June 21, 1746,							31 8 32,7
Mean zenith distance of the Sun's center, June 21, 1746,							28 11 4,1
Mean distance of Arcturus from the Sun's center, 1746,							2 57 28,6

	Observed zenith d. of Arcturus.	Batho- meter.	Thermo- meter.	Refrac- tion.	Aber- ration.	Nuta- tion.	Precef- sion.	Observations reduced.
1766.	° ' "			"	"	"	"	° ' "
May 13.	31 14 20,7	29,46	43	34,2	-1,9	-7,6	+2	31 14 48,1
21.	31 14 23,1	29,81	46	35,1	-0,2	-7,6	+1,7	31 14 52,1
22.	31 14 19,9	29,67	49	34,7	0	-7,6	+1,6	31 14 48,6
June 23.	31 14 20,8	29,90	70	33,1	+6,4	-7,7	-0,1	31 14 52,5
Mean								31 14 50,3
Error of the line of collimation								+ 2,6
Mean zenith distance of Arcturus, June 21, 1766, . .								31 14 52,9
Mean zenith distance of the Sun's center, June 21, 1766,								28 11 16,6
Mean distance of Arcturus from the Sun's center, 1766,								3 3 36,3

From the foregoing observations, it appears that the mean solstitial zenith distance in summer was as follows.

Variation in 100 years.

	"	"
1743.	28 11 2,8	60
1746.	28 11 4,1	62,5
1748.	28 11 4,1	69,4
1766.	28 11 16,6	

And, by comparing the three former with the latter, the variation of the obliquity of the ecliptic in 100 years is as is expressed in the last column of the table.

By comparing the distance of Arcturus from the center of the Sun in 1743, with the same distance as observed in 1766 (an allowance being made for the proper motion of the star during the interval, as also for its variation in declination arising from the precession

cession of the equinoxes), it appears that its distance is $17'',3$ less than it would have been, if the distance of the Sun's center from the equator had remained unvaried. By that quantity, therefore, the obliquity of the ecliptic has altered in 23 years; which is at the rate of $75'',2$ in 100 years.

By comparing, in like manner, the distance in 1746, the obliquity of the ecliptic has diminished $15'',6$ in 20 years, or $78''$ in 100 years.

Distance in 1743	2 56 32,8	In 1746. 2 57 28,6
Motion of the star in decl. Southward	7 20,8	
Computed distance in 1766	3 3 53,6	3 3 51,9
Observed distance in 1766	3 3 36,3	3 3 36,3
Variation of obliquity	17,3	15,6

The foregoing deductions prove, I think, beyond all doubt, that the obliquity has become less; but as the interval of time between the two terms of comparison is so short, that the errors committed in observing may bear a sensible proportion to the small quantities just now found, and which, perhaps, are somewhat too large; let us have recourse to Mr. Flamsteed's observations, and compare them with observations made by myself, in the course of the last and present years. For this purpose, I have reduced all the observations of the Sun, made in 1690, from May 26 to June 24, O. S. and also all the observations of Arcturus, made in the same year, to their mean position at the summer solstice of that year. The observations, together with my own made at Oxford, are as follows.

		Observed zen. dist. of the Sun's limbs.			Refra- ction.	Sun's semi- diameter.		Dist. from foliice.			Observations reduced.		
1690.		o	i	''	''	i	''	o	i	''	o	i	''
May 26.	Lo. L.	29	6	20	31,8	15	46,4	0	50	26,1	28	0	39,3
	Up. L.	28	34	50	31,1	15	46,1						41,4
June 2.	Lo. L.	28	31	35	31,1	15	45,7	0	15	28,3			52,1
	Up. L.	28	0	5	30,4	15	45,7						52,8
3.	Lo. L.	28	28	20	31	15	45,6	0	12	8,6			56,8
	Up. L.	27	56	45	30,3	15	45,6						51,1
4.	Lo. L.	28	25	15	30,9	15	45,5	0	9	10,9			49,5
	Up. L.	27	53	55	30,3						28	0	59,9
6.	Lo. L.	28	20	40	30,8	15	45,5	0	4	30,1	28	0	55,2
	Up. L.	27	49	15	30,2						28	1	0,6
7.	Up. L.	27	47	10	30,1	15	45,4	0	2	46,8	28	0	38,7
	Lo. L.	28	18	40	30,8						28	0	38,6
10.	Lo. L.	28	16	10	30,7	15	45,3	0	0	5,5	28	0	49,9
	Up. L.	27	44	40	30,1						28	0	49,9
12.	Up. L.	27	44	50	30,1	15	45,3	0	0	22	28	0	43,4
	Lo. L.	28	16	45	30,8						28	1	8,5
13.	Lo. L.	28	17	17,5	30,8	15	45,1	0	1	7,4	28	0	55,8
	Up. L.	27	45	50	30,1						28	0	58,8
14.	Up. L.	28	13	30	30,8	15	45,1	0	2	17,6	28	0	58,1
	Lo. L.	27	46	50	30,1						28	0	47,6
16.	Up. L.	27	50	40	30,2	15	45,1	0	3	51,8	28	1	3,5
	Lo. L.	28	22	15	30,9						28	1	9
17.	Up. L.	27	53	5	30,2	15	45	0	8	16,4	28	1	3,8
	Lo. L.	28	24	30	30,9						28	0	59,5
20.	Lo. L.	28	34	15	31,1	15	45	0	17	56,4	28	1	4,7
	Up. L.	28	2	45	30,4						28	1	4
25.	Up. L.	28	21	5	30,8	15	45,1	0	36	30,6	28	0	50,3
Mean											28	0	54,2
Error of the line of collimation												1	30
Sun's parallax											27	59	24,2
												—	4,1
Nutation											27	59	20,1
												+	9,5
Mean solstitial zen. dist. of the Sun's center, June 11, 1690, O. S.											27	59	29,6

	Observed zenith dist. of Arcturus.			Refrac- tion.	Aber- ration.	Nuta- tion.	Precef- sion.	Observations reduced.		
1690.	°	'	"	"	"	"	"	°	'	"
Feb. 14.	30	39	20	33,8	—12,2	—5,1	—2,3	30	39	34,2
Apr. 13.	30	39	20	33,8	—5,5	—5,3	—5,4	30	39	37,6
25.	30	39	10		—3,3	—5,4	—6			29,1
26.	30	39	15		—2,9	—5,4	—6,1			34,4
May 13.			10		+0,4	—5,5	—6,9			31,8
14.			10		+0,6	—5,5	—7			31,9
15.	30	39	10	33,8	+0,8	—5,5	—7			32,1
20.			10		+1,9	—5,5	—7,3			32,9
22.			10		+2,3	—5,5	—7,4			33,2
24.	30	39	5		+2,7	—5,6	—7,5			28,4
June 12.			10	33,8	+6,4	—5,6	—8,5			36,1
13.			10		+6,6	—5,6	—8,6			36,2
16.	30	39	10		+7,1	—5,6	—8,7			36,6
17.			10		+7,2	—5,6	—8,8	30	39	33,4
July 1.	30	39	10	33,8	+9,4	—5,7	—9,5	30	39	30,6
Dec. 13.	30	39	35	33,8	—6,6	—6,5	—18,1	30	39	37,6
14.	30	39	40	33,8	—6,8	—6,5	—18,2	30	39	42,3
Mean, January 1, 1690, O. S.								30	39	34
Precession to June 11, 1690										+ 8,4
Mean zenith distance of Arcturus, June 11, 1690 . .								30	39	42,4
Mean solstitial zenith distance of the Sun's center, } June 11, 1690 }								27	59	29,6
Mean distance of Arcturus in declination from the Sun's } center, 1690 }								2	40	12,8

	Observed zenith distance of the Sun's upper limb.	Barometer.	Thermometer.	Refraction.	Sun's semi-diameter.	Dist. from solstice.	Observations reduced.
	° ' "			"	' "	° ' "	° ' "
1771. June 8.	28 36 47,6	30,08 $\frac{1}{2}$	64 $\frac{1}{3}$	30,9	15 48	0 36 0,4	28 17 6,1
14.	28 11 38,1	29,94	63 —	30	15 47,4	10 46,6	8,9
18.	28 3 4,4	29,75	61 +	29,7	15 47,2	2 8,3	13
21.	28 0 55,4	30,08 $\frac{2}{3}$	65 $\frac{1}{3}$	29,7	15 47	0 0,8	11,3
22.	28 1 1,4	30,07	66 —	29,6	15 47	0 7,6	28 17 10,4
24.	28 2 26,1	30,07	62	30	15 46,9	1 35,8	7,2
25.	28 3 46,9	30,05	69	29,5	15 46,9	2 51,7	6,5
26.	28 5 38,1	29 97 $\frac{1}{2}$	68	29,5	15 46,9	" " "	11,8
29.	28 13 21,3	29,84	61 —	30,2	15 46,9	" 7,7	28 17 10,7
Mean							28 17 8,7
Sun's parallax							4,1
Nutation							28 17 4,6 — 6,8
Error of the line of collimation							28 16 57,8 + 4,8
Mean solstitial zenith distance of the Sun's center, 1771.							28 17 2,6

	Observed zenith distance of the Sun's limbs.			Baro- meter.	Ther- mo- meter.	Refrac- tion.	Sun's semi- diameter.	Dist from collim.	Corrections included.
	° ' "					"	"	"	"
1772.									
June 8.	Lo. L.	29	4 31.3	30,18	71	30,7	-15 48	0 31 59,9	28 17 14,1
9.	Up. L.	28	28 1	30,25 ¹	67 ¹ ₂	30,1	+15 47,9	27 5,4	21 17 13,6
11.	Lo. L.	28	51 5,5	30,10 ¹ ₂	71 ¹ ₄	30,3	-15 47,7	18 29,2	18,9
12.	Lo. L.	28	47 15,7	30,22 ¹ ₂	60 ¹ ₂	31,2	-15 47,6	14 48,4	10,9
14.	Up. L.	28	9 34,1	30,15 ¹ ₂	63	30,1	+15 47,4	8 38,4	13,5
	Lo. L.	28	41 11,1			30 8			16,1
15.	Up. L.	28	7 5,4	29,9 ¹ ₂	72	29,2	+15 47,3	6 10,7	11,2
	Lo. L.	28	38 40,8			29,8			28 17 12,6
16.	Up. L.	28	5 0,5	29,82 ¹ ₂	70 ¹ ₄	29,1	+15 47,3	4 7,6	9,1
	Lo. L.	28	36 38,6			29,8			13,5
18.	Up. L.	28	2 16,4	30,02 ¹ ₄	71 ¹ ₂	29,2	+15 47,2	1 15,6	17,2
	Lo. L.	28	33 48,1			29,7			15
19.	Up. L.	28	1 26,3	29,97	75 ¹ ₃	28,8	+15 47,1	0 26,8	15,4
	Lo. L.	28	32 55,8			29,5			11,4
20.	Lo. L.	28	32 30,3	29,93	79	29,1	-15 47,1	0 2,8	28 17 9,5
21.	Lo. L.	28	32 32,8	29,79	72	29,6	-15 47	0 3,5	11,9
22.	Up. L.	28	1 24,8	30,00 ¹ ₄	73	29	+15 47	0 29,9	28 17 10,9
23.	Up. L.	28	2 14,4	30,01	76 ¹ ₄	28,8	+15 47	1 19,4	28 17 10,8
	Lo. L.	28	33 54,1			29,5			17,2
26.	Up. L.	28	7 20,3	29,96	85 —	28,2	+15 46,9	6 18,9	16,5
	Lo. L.	28	38 50,4			28,8			28 17 13,4
Mean									28 17 13,4
Sun's parallax									— 4,1
Nutation									28 17 9,3 — 8,7
Error of the line of collimation									28 17 0,6 + 4,8
Mean solstitial zenith distance of the Sun's center, 1772,									28 17 5,4

	Observed distance of Arcturus.	Distance of meter.	Thermometer.	Refraction.	Aber-ration.	Nuta-tion.	Precef-sion.	Observations reduced.
1772.	0 1 1			"	"	"	"	0 1 "
July 11.	31 21 53,6	29,87	73	52,9	+ 9,4	+ 1,9	- 10,1	31 22 27,7
Aug. 11.	31 21 55,6	29,91	64	53,8	+ 12,2	+ 2,1	- 11,7	33
17.	50,6	29,93	75	52,8	+ 12,2	+ 2,1	- 12	25,7
19.	54,6	29,76	77	52,5	+ 12,2	+ 2,2	- 12,1	29,5
Oct. 16.	31 21 58,1	29,99 ¹ / ₄	61	54,2	+ 7,7	+ 2,6	- 15,1	27,2
Nov. 2.	31 22 3,3	29,32 ¹ / ₄	49	54,5	+ 4,4	+ 2,7	- 16	28,9
7.	22 3,5	29,25	49	54,3	+ 3,8	+ 2,7	- 16,3	28
12.	8,3	29,82	38	54,9	+ 2,1	+ 2,7	- 16,5	32,5
20.	31 22 12,3	29,85	46	54,1	+ 0,4	+ 2,7	- 16,9	31 22 32,6
21.	10,3	29,52 ¹ / ₄	45	55,4	+ 0,2	+ 2,7	- 17	31,6
23.	11,3	29,37	40	55,2	- 0,2	+ 2,7	- 17,2	31,8
Dec. 10.	15,3	29,62	39 ¹ / ₂	55,7	- 3,9	+ 4,9	- 18	32
16.	31 22 12,3	29,69	45 ¹ / ₂	55,1	- 5	+ 2,9	- 18,3	31 22 27
Mean zenith distance of Arcturus, January 1, 1772								31 22 29,8
Precession to June 21, 1772								+ 9
Mean zenith distance of Arcturus, June 21, 1772								31 22 38,8
Error of the line of collimation								+ 4,2
True mean zenith distance of Arcturus, June 21, 1772								31 22 43
Mean zenith distance of the Sun's center, June 21, 1772								28 17 54
Mean distance of Arcturus in declination from the Sun's center, } June 21, 1772 }								3 5 37,6
0 1 "								
Mean distance in June 1690								2 40 12,8
Precession, &c. to June 1772								26 16,4
Computed distance in June 1772								3 6 29,2
Observed distance in June 1772								3 5 37,6
Diminution of obliquity in 82 years								51,6

From the foregoing observations it appears that, at the summer solstice of the year 1690, Arcturus was $2^{\circ} 40' 12''.8$ to the South of the Sun's center in declination: the motion of the star, in declination, from that time to the summer solstice of the year 1772, including its proper motion, is $26' 16''.4$. Arcturus, therefore, in 1772, should have been $3^{\circ} 6' 29''.2$ to the South of the Sun's center, if the angle of the ecliptic and equator had not varied: but that distance was found by actual observation to be $51''.6$ less. By so much therefore must the obliquity of the ecliptic have become less in an interval of 82 years; and, consequently, the variation in 100 years will be $62''.92$.

If the observations of Arcturus be reduced to the solstice of 1771, and the zenith distance of the Sun's center, as observed in that year, be made use of in the same manner, the variation of the obliquity in 81 years will be found $= 48''.8$, and in 100 years $= 60''$.

If the quantity of the arc of Mr. Flamsteed's instrument were accurately known, the observations which he made at the winter solstice in 1690 might be compared with later observations, in order to determine both the quantity of the obliquity in 1690, and also the variation since his time. Accordingly, I have endeavoured to determine the error of the arc of the instrument between 28° and 75° of zenith distance, and proceeded in the following manner. I computed several observations of the stars ζ Tauri, η Pleiadum, η and μ Geminorum, and ϕ , σ , and \circ Sagittarii, as observed by Mr. Flamsteed, in the years 1690, 1691, and 1692, and reducing them

to the years 1760 and 1766, I compared the differences of declination between those stars, resulting from Mr. Flamsteed's observations, with the differences given by the places of the same stars, as settled by Dr. Bradley in 1760, and also by actual observations of the same stars made at Shirburn-Castle in 1766; and, by combining these differences together, I found that the whole arc of 90 degrees was too short by 43''. Supposing the error to be uniform, the proportional part of this quantity, thus found for the solstitial zenith distance of the Sun in June = 13'',4, is nearly confirmed upon the authority of Mr. Flamsteed himself, who, in the *prolegomena* to the third volume of the *Historia Cœlestis*, where he is deducing the latitude of the Royal Observatory at Greenwich, and the quantity of the obliquity in 1690, from his own observations, allows the zenith distances at 28°, 36°, and 40°, on his instrument, to be too small by 15' and by 20'', at 75°.

I have therefore computed the observations of the Sun, made from November 30 to December 20 of 1690, which, reduced to the solstice, are as in the following table; to which are subjoined the observations made by myself at Oxford, at the winter solstice of 1771.

	Observed zenith distance of the Sun's limbs.	Refraction. +	Sun's semi- diameter.	Dist. from solstice. +	Observations reduced.
	° ' "	' "	' "	° ' "	° ' "
1690.					
Nov. 30.	Lo. L. 74 45 17,5	3 27,9	— 16 16,6	0 25 43,1	74 58 11,9
	Up. L. 74 12 40	3 19,6	+ 16 16,6		74 57 59,3
Dec. 2.	Lo. L. 74 54 5	3 28,7	— 16 16,6	17 16,1	74 58 33,2
	Up. L. 74 21 25	3 21,7			19,4
10.	Lo. L. 75 11 0	3 32,7	— 16 17,2	0 4,6	20,1
	Up. L. 74 38 35	3 25,4			22,2
13.	Lo. L. 75 9 50	3 32,4	— 16 17,2	1 24,4	74 58 29,6
	Up. L. 74 37 15	3 25,3			21,9
15.	Lo. L. 75 6 45	3 31,6	— 16 17,3	4 39,4	74 58 38,7
	Up. L. 74 34 5	3 24,5			26,2
17.	Lo. L. 75 1 30	3 30,2	— 16 17,3	9 46,9	29,8
20.	Up. L. 74 13 15	3 19,8	+ 16 17,3	25 38,8	74 58 30,9
	Lo. L. 74 46 5	3 27	— 16 17,3		74 58 53,5
Mean					74 58 25,9
Error of the line of collimation					— 1 10
Sun's parallax					74 57 15,9 — 8,5
Nutation					74 57 7,4 — 9,6
Mean solstitial zenith distance of the Sun's center, Dec. 1690,					74 56 57,8

	Observed zenith distance of ☉'s upp. limb.	Baro- meter.	Ther- mome- ter.	Refrac- tion.	Sun's semi- diameter. +	Dist. from solstice. +	Observations reduced.
1771.	° ' "			" "	" "	° ' "	° ' "
Dec. 8.	74 11 1,7	29,43	44 $\frac{1}{2}$	3 21	16 18	0 42 26,3	75 13 12,2
12.	74 32 25,4	29,56 $\frac{1}{3}$	47 +	3 25,2	16 18,5	21 3,4	17,7
16.	74 46 32,3	28,68	45 $\frac{2}{3}$	3 22,7	16 18,7	7 2,5	21,8
19.	74 51 57,3	29,23	42	3 29,6	16 19	1 26,1	17,2
24.	74 51 48,5	29,07	40	3 29,6	16 19,1	1 31	13,4
25.	74 50 24,2	29,43	41 $\frac{1}{2}$	3 31,2	16 19,2	2 56,9	16,7
27.	74 46 13,3	29,42 $\frac{1}{2}$	48 $\frac{1}{2}$	3 26,6	16 19,2	7 13,4	17,7
30.	74 36 13,9	30,18	39 $\frac{1}{2}$	3 34,1	16 19,3	17 9,3	75 13 21,8
Mean							75 13 17,3
Sun's parallax							— 8,5
							75 13 8,8
Refraction							+ 7,9
							75 13 16,7
Error of the line of collimation							+ 4,8
Mean solstitial zenith distance of the Sun's center, December 1771,							75 13 21,5

The mean obliquity of the ecliptic resulting from the zenith distances, as observed at the two solstices in 1690, by applying the known latitude of the place, will be found to be widely different, if no correction be applied for the error of the instrument.

June, zenith distance	—27 59 29,6	Dec. zen. dist.	74 56 57,8
Latitude of Greenwich	51 28 38		—51 28 38
	23 29 8,4		23 28 19,8

But if the observations be corrected by the error of the instrument, the two results will be found to agree together as nearly as can be expected.

"			"		
27	59	29,6	74	56	57,8
	+	13,4		+	35,8
			<hr/>		
-27	59	43	74	57	33,6
51	28	38	-51	28	38
			<hr/>		

Obliquity 23 28 55 23 28 55,6

Or, if the obliquity be required independent of a knowledge of the latitude of the place, it will be found to be $= 23^{\circ} 28' 55'',3$.

December	74	57	33,6
June	-27	59	43
	<hr/>		

Difference 46 57 50,6
Mean obliquity 1690, $\frac{1}{2}$ Diff. 23 28 55,3

By comparing the observations at the summer solstices of 1771 and 1772 with those at the winter solstice of 1771, it appears that the mean obliquity was about the beginning of the year 1772 $= 23^{\circ} 28' 9'',4$ and $23^{\circ} 28' 8''$. I suppose therefore the mean obliquity to be $23^{\circ} 28' 8''$ at the beginning of the present year; and consequently, the obliquity has diminished, by my observations, $47''$ in 81 years, since Mr. Flamsteed's time, or at the rate of $58''$ in 100 years, a quantity which will be found nearly at a mean of the computations framed by Mr. Euler and Mr. de la Lande, upon the principles of attraction.

Oxford, Dec. 23, 1772.

XV. *New Observations upon Vegetation.*
By Mr. Mustel of the Acad. of Sciences
at Rouen; translated from the French.

Read Jan. 14, 1773. **M**ANY celebrated writers, induced by the analogy, which they observed betwixt the vegetable and animal kingdoms, have admitted the circulation of the sap in the one, in a similar manner to the circulation of the blood in the other.

This important point of vegetable œconomy produced a diversity of opinions, and has not yet been sufficiently cleared up.

Dr. Hales, in his *Vegetable Staticks*, does not seem to embrace the system of the circulation of the sap; but he does not prove the contrary*.

* *Il ne prouve pas contre.* This certainly is a mistake. Dr. Hales, in the IVth Chapter of his *Physical Staticks*, not only declares openly against the doctrine of the circulation of the sap, and overturns the arguments alledged in favour of this opinion; but he produces several new experiments, which prove directly the impossibility of such a circulation. (See p. 144, &c.) His reasons have been thought so convincing, that the system of the circulation in plants has been ever since exploded in England; and that they have had a similar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, intitled *Recherches sur l'usage des feuilles*, printed in 1754, p. 269. "Pour moi, persuadé de la fausseté de cette opinion (que la sève circuloit comme le sang) par les expériences de M. Hales (Ch. IV.) &c." M. M.

Mr.

Mr. Du-Hamel, in his *Physiology of Trees*, contents himself with relating what has been said for or against this opinion; but, though he sufficiently hints that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals; for want of such an organ, they were forced to imagine valves and paps in the lymphatick vessels of plants, by means of which the liquors once introduced into the sap vessels were supposed to be hindered from going back; but, unfortunately, no body has ever been able to discover these valves and paps, so different from the simple contrivances, by which nature is used to arrive at her ends.

An experiment, which I made, and of which I propose giving an account in this paper, throws a great light upon this question, as well as upon several others; and the conclusions deducible from it appear to me decisive.

On the 12th of January, I placed several shrubs in pots against the windows of my hot-house, some within the house, and others without it. Through holes made for this purpose in the panes of glass, I passed a branch of each of the shrubs, so that those on the inside had a branch without, and those on the outside one within; after this, I took care that the holes should be exactly closed and luted. This inverse experiment, I thought, if followed closely, could not fail affording sufficient points of comparison, to trace out the differences, by the observation of the effects.

The 20th of January, a week after this disposition, all the branches that were in the hot-house began to disclose their buds. In the beginning of February, there appeared leaves, and towards the end of it, shoots of a considerable length, which presented the young flowers. A dwarf apple-tree and several rose-trees, being submitted to the same experiment, shewed the same appearance then as they commonly put on in May; in short, all the branches which were within the hot-house, and consequently kept in the warm air, were green at the end of February, and had their shoots in great forwardness. Very different were those parts of the same tree, which were without and exposed to the cold. None of these gave the least sign of vegetation; and the frost, which was intense at that time, broke a rose pot placed on the outside, and killed some of the branches of that very tree, which, on the inside, was every day putting forth more and more shoots, leaves, and buds, so that it was in full vegetation on one side, whilst frozen on the other.

The continuance of the frost occasioned no change in any of the internal branches. They all continued in a very brisk and verdant state, as if they did not belong to the tree, which, on the outside, appeared in the state of the greatest suffering. On the 15th of March, notwithstanding the severity of the season, all was in full bloom. The apple-tree had its root, its stem and part of its branches, in the hot-house. These branches were covered with leaves and flowers; but the branches of the same tree, which were carried to the outside, and exposed to the cold air, did not in the least
partake

partake of the activity of the rest, but were absolutely in the same state, which all trees are in during winter. A rose tree, in the same position, shewed long shoots with leaves and buds; it had even shot a vigorous branch upon its stalk, whilst a branch which passed through, to the outside, had not begun to produce any thing, but was in the same state with other rose-trees left in the ground. This branch is four lines in diameter, and eighteen inches high.

The rose-tree on the outside was in the same state; but one of its branches drawn through to the inside of the hot-house, was covered with leaves and rose-buds. It was not without astonishment that I saw this branch shoot as briskly as the rose-tree which was in the hot-house, whose roots and stalk, exposed as they were to the warm air, ought, it should seem, to have made it get forwarder than a branch belonging to a tree, whose roots, trunk, and all its other branches were at the very time frost-nipt. Notwithstanding this, the branch did not seem affected by the state of its trunk; but the action of the heat upon it produced the same effect as if the whole tree had been in the hot-house.

It would be useless to give an account of the diary I kept throughout the course of this interesting experiment. It may be sufficient to observe, that the walk of nature was uniformly the same. The interior branches continued their productions in a regular manner, and the external ones began theirs at the same time, and in the same manner, as they would have done, had they been left in the ground. The fruits of the in-

terior branches of the apple-tree were, in the beginning of May, of the size of nutmegs; whilst the blossoms but just began to shew themselves on the branches without. I shewed Mr. Du-Tillet, of the Academy of Sciences at Paris, on his passage through this town, the effects of my experiments, and likewise communicated to him another observation, which chance occasioned, and ought not to be omitted.

I observed that three of the flower buds of the apple-tree had been gnawed off by a snail in such a manner, that all the petals and stamens had disappeared, being eat up close to the calyx. This not having been entered by the snail, the basis of the pistillum and the embryo were preserved.

I took it for granted that these flowers would bear nothing; but I was soon convinced of my mistake. Almost all of them bore fruit; the apples were perfectly formed, and six or seven pretty large ones too were seen upon each bunch. On the other hand, the snail had spared some other bunches, (doubtless because more difficult to be got at;) but out of ten or twelve flowers in each bunch, not above one or two shewed any signs of fruit. This suggested to me the idea, that, when the flowers of trees are full blown, the prevention of the natural fall of the petals and stamens gives a greater assurance of the fructification; and on several times repeating the following experiment, I convinced myself that it did so. In imitation of the snail, I cut with my scissars the petals of apple, pear, plumb, and cherry blossoms, close to the calyx. Almost every one of those,
which

which were thus cut, succeeded, whilst several of the neighbouring flowers mis-carried.

Thus did a snail teach me how to render a tree fruitful ; nor is it the first time that animals have been the instructors of mankind. I confess, however, that this process is not very practicable in a large orchard : but it might be adopted in an espalier ; in which one would chuse to procure a great deal of fruit from trees of the best sort.† It may indeed be questioned, whether the suppression of the stamens would not render the fruit barren ; and in fact I found, that, though the flowers of the dwarf apple-tree, whose petals and stamens were eat up by the snail, gave me apples equally large and beautiful, and that, when I came to open them, I found the capsules formed as usual at the center of them ; yet they were entirely empty, without the least appearance of a pip. Absolute fructification consequently did not take place ; since botanists, with reason, call nothing fruit but the seed, which contains the germen, which is to perpetuate the species. All the other parts, being only intended to co-operate in the formation and preservation of the seeds, perish of course, when once the seeds are come to maturity and perfection, and the work of nature fulfilled.

Another remarkable thing in these apples is, that in the upper part there was found a much deeper cavity than usual. It was eight or nine lines deep. The orifice of this cavity was bordered by five tubercles, indented and somewhat elevated ; but there was no vestige of the calyx, which, it is well known, remains always to the

upper part of apples and pears, and is commonly called the eye.

I now return to my first experiment; the consequences of which, as I have described them, seem to prove,

I. First that the circulation of the sap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following observations.

The tree in the hot-house went through all its changes during the winter, and the branch exposed to the open air underwent none; consequently the sap, which was in action in the root, stock, and head, of the tree, did not circulate through the branch without; which had no share in the vegetation of the roots and trunk. It might, indeed, be argued that the cold air, to which this branch was exposed, stopped the circulation, and therefore that the first experiment would not be decisive; but the inverse of it seems fully so.

The tree placed on the outside of the hot-house continued, during the whole winter, in the state of numbness, natural to all trees, which are exposed at that season; but one of its branches, which was in the hot-house, put forth successively its buds, leaves, blossoms, and fruits. Whilst therefore the root of the tree, to which this branch belonged, was in the ground so frozen, that the pot itself, in which it stood, was broken by it, whilst the stock and top of the tree were so covered over with ice, that many of the branches were killed; this branch alone did not in the least partake of the common state of numbness and suffering, and was on the contrary in full vegetation.

tion. The sap in it must have been extremely rarefied, and in very quick motion, whilst that of the tree was greatly condensed, and in total inaction. How is it possible to conceive a circulation of the sap from such a frozen root and stock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment must appear conclusive against the system of circulation; since in this case it could at best only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which should be confined to one limb.

II. This experiment proves, that each part of a tree is furnished with a sufficient quantity of sap to effect the first production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-house should have derived its sap from the roots of the tree: as they, at that time, lay in a very small quantity of earth, rendered extremely hard and dry by the frost, they could have but little liquor to spare; and even this, considering the congealed state of the lymphatick vessels of the stock, could have found no passage to the branch. This branch must of course have been enabled to continue its vegetation by the quantity of sap with which it was provided, the consumption of which must have been supplied at the first breaking of the frost. This truth, now demonstrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have observed that a tree, which has been blown down in autumn, though separated from its trunk, begins the same vegetation, that it would have done if it had remained standing.

ing. Its buds open, it bears leaves, and even shoots, which sometimes are very long, and must be the effects of the sap it contained. It is true, indeed, that this appearance does not continue long, because the provision of sap once exhausted, without being renewed, every thing must of necessity perish.

An effect of the like kind often deceives us in trees, that have been newly planted, and in scions, which produce flowers and even fruits, without ever having taken root. But in this case the symptoms, which would seem to promise life, are on the contrary the forerunners of death; because the leaves, being from their nature the most powerful organs of transpiration and dissipation, the graft is the more readily exhausted, when there is no root to furnish it with a fresh supply of nutritive juices.

III. This experiment proves that it is heat, which unfolds the leaves, and produces the other parts of fructification, in the branch exposed to its action.

Autumn is the time, in which Nature employs itself as it were clandestinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, blossoms, and fruits, that are to be produced in the course of the succeeding summer. These buds prepare and work themselves out, during the winter, under the rough coats, that are destined to preserve them from the injuries of the weather. As soon as the warm weather in the spring begins to be felt, the buds open, and their coats, which then become useless, drop off, and give place to the productions, which
they

they contained and preserved. Immediately after this, the blossoms, flowers and fruits make their appearance. This is the usual operation; but in the case before us, nature was, as it were, surprized by art: what she should not have done till spring, she did in the winter, because the heat of the hot-house produced that expansion, which, according to the natural course, ought to have been effected by the rays of the sun darting less obliquely than before upon the horizon. There is no doubt but it is to heat, either natural or artificial, that this expansion is owing; and the experiment proves that it is only in that part of the tree, which is exposed to the effect of heat, that the sap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this, it appears that the vegetable œconomy is different from the animal, and that those, who endeavoured to establish the circulation in both, carried their analogy too far.

This fact, now established, furnishes a good reason why in the tapping of the maple and sugar-birch-trees, so much liquor runs out on one side, and none at all on the other. It is well known that, if during the time of a frost, or a summer's day, towards noon, you bore a hole on the side of the maple-tree exposed to the south, you will get a great quantity of liquor from it; and that if you bore the north-side at the same time, you will not get a drop. The cause of this evidently appears from what has been said. One likewise sees why trees exposed to the south lose a great many of their branches, and sometimes die altogether, in the course of a severe winter; whilst

trees of the same sort, but placed to the north, or in some other exposition, will stand the hardest frosts. This is particularly remarkable in the ever-greens, whose resinous and oily sap being liquefied by the heat of the sun, the tree cannot escape suffering a great deal, whenever it is surprized in that state by the night frosts. Those observers, who attend to this, and know how well pines, firs, and bays succeed, when planted on the back of mountains exposed to the north, will take care not to place such kind of trees in a southern aspect, in hopes of their succeeding better by it.

Many other consequences might be drawn from these experiments; but the bounds, I have assigned to this paper, do not allow it. I propose examining them more at large in a treatise upon vegetation, which, I hope, the observations and experiments I have made, may render interesting and useful.

XVI. Actual Fire and Detonation produced by the Contact of Tin-foil, with the Salt composed of Copper and the Nitrous Acid. By B. Higgins, M. D. Communicated by R. Brocklesby, M. D. F. R. S. and of the Royal College of Physicians in London.

Read Dec. 24, 1772, and Jan. 28, 1773. **S**EVERAL pieces of thin sheet-copper, placed vertically, and at a small distance from each other, in the strong nitrous acid diluted with half its quantity, or more, of water, and suffered to remain in a close vessel, until the acid is saturated, afford a crystalline blueish green salt, which is to be separated from the undissolved copper and the superfluent green liquor, and kept in a well-corked bottle; because, on exposure to the air, it deliquesces.

This salt, taken moist, but not very wet, and beaten to the fineness of basket sea-salt, in a mortar, is to be strewed to the thickness of a shilling, on a piece of tin-foil, twelve inches in length and three in breadth.

Then the foil is to be instantly rolled up, so as to include the salt, as it lay, between the coils. The ends are to be shut by pinching them together,

ther, and the whole is to be pressed flat and close.

All this being done as quickly as possible, the first phænomenon is—A part of the salt deliquesces.

2d. This part, impregnated with tin, changed in colour, and of a thicker consistence, begins to froth forth from the ends of the coil.

3d. A strong frothing, accompanied with moderate warmth.

4th. The emission of copious nitrous fumes.

5th. Heat intolerable to the fingers.

6th. Explosion and fire, which burst and fuse the tin-foil in several places, if it be very thin.

After many conjectures and experiments, I discovered a property in the cupreous salt, from which; and the known affinities of the bodies concerned, these appearances, however new and singular, may be accounted for.

The cupreous salt properly dried and placed where it may receive a heat, not much greater than what the hand can bear, takes fire. The circumstances which favour this ignition, and contribute to produce it in the smallest degree of heat, concur in the following convenient method of trying the experiment.

A piece of soft bibulous paper is to be dipped in the nitrous solution of copper, and dried before the fire two or three times alternately. Then it is to be approached towards the heat, as near as can be borne, by the hand which holds it without pain: there, if it has been sufficiently dried, it will presently catch fire, and burn to a brown calx.

The

The easy ignition of the salt in a slight heat being thus ascertained, there is no room to doubt that the foregoing phænomena are produced in the following manner.

The acid of the liquor, which moistened the salt, quits the copper to unite with the tin, leaving the water to be imbibed by the contiguous salt of copper, which then dissolves, and acts briskly on the tin-foil.

It is well known that the action of the nitrous acid on tin is always accompanied with considerable heat and effervescence, and that the solution of metallic salts in watry liquors is hastened by heat.

In our experiment, the warmth generated by the first action of the cupreous solution, promotes the deliquescence of the crystallized salt. The union of the acid with the tin is rapid, not only as being assisted by heat, but on account of the great surface exposed; whence the strong frothing, and the extraordinary heat, by which the redundant moisture is carried away, and the undecomposed part of the cupreous salt, together with that lately formed with the tin, perfectly dried.

The heat generated upon both surfaces of a large expanse of tin, is concentrated by closely coiling it into a small compass, and being retained by the various surrounding laminæ of metal, it is necessarily accumulated to a quantity, which, if we may judge from the touch, is more than sufficient to fire the dry cupreous salt.

The salt formed with tin, and the nitrous acid, burns and sparkles in a red heat. Catching fire, therefore, from the ignited cupreous salt, it burns

with it, and assists in the detonation, which is common to all nitrous compositions in similar circumstances.

If the salt be very wet, there will not be much fire or explosion, because the heat will be dissipated before the salt can be sufficiently dried in every part.

If the salt be not moist, it cannot commence the action which is necessary; and there will be no fire, because there can be no hasty solution of the tin to give the requisite heat.

If the tin and salt be not coiled up in due time, there will be very little heat and no fire; because the dissipation of the heat from a broad expanse, keeps pace with the generation of it; and as the moisture exhales quickly in this manner, there is none left to renew the action on the tin and consequent heat, when the proper time of coiling has elapsed.

A piece of tin-foil, larger than that I have described, cannot easily be managed; smaller pieces give less fire in the direct proportion of their surfaces, and the quantity of salt which they can, at the same instant, reduce to the required state of dryness.

The sudden dissipation of the moisture appears to me the most curious of these phenomena. To render it the more observable, I made the following experiments.

I placed a piece of tin-foil, twelve inches long by two broad, loosely coiled, and standing vertically on the flattest end, in half a table-spoonful of the saturated solution of copper in the diluted nitrous acid, and found that scarce five seconds elapsed

elapsed from the time, when a brisk effervescence, accompanied with weak nitrous fumes, arose, until the liquor became a consistent mass, and sparks of fire issued from the coils of tin; which having attracted part of the solution above the common level, brought it into the condition in which it is readily dried, heated, and fired.

A like quantity of the same solution, kept in a strong boiling heat, does not acquire such consistence in a ten-fold space of time.

The hasty exhalation, therefore, is not caused by the heat alone; neither does it seem to require any great surface. What else it is owing to, I commit for a while to the examination of the curious.

Orchard-Street, Portman-Square, Dec. 23, 1772.

B. Higgins.

Received

Received Nov. 12, 1772.

XVII. *Extracts of some Letters, from Sir William Johnson Bart. to Arthur Lee, M. D. F. R. S. on the Customs, Manners, and Language of the Northern Indians of America.*

Read Jan. 28, 1773. **I**N all enquiries of this sort, we should distinguish between the more remote tribes, and those Indians, who, from their having been next to our settlements for several years, and relying solely on oral tradition for the support of their antient usages, have lost great part of them, and have blended some with our customs, so as to render it extremely difficult, if not impossible, to trace their customs to their origin.

The Indians did certainly live under more order and government formerly, than at present. This may seem odd, but it is true; for, their intercourse being with the lower class of our traders, they learn little from us but our vices; and their long wars, together with the immoderate use of spirituous liquors, have so reduced them, as to render that order, which was first instituted among them, unnecessary and impracticable.

They

They do not at present use hieroglyphics; their figures being drawn, to the utmost of their skill, to represent the thing intended. For instance, when they go to war, they paint some trees with the figures of warriors, often the exact number of the party; and if they go by water, they delineate a canoe. When they gain a victory, they mark the handle of their tomahawks with human figures, to signify prisoners; and draw the bodies without heads, to express the scalps they have taken. The figures which they affix to deeds have led some to imagine, that they had alphabetical characters or cyphers. The fact is this. Every nation is divided into tribes, of which some have three, as the turtle, bear, and wolf; to which some add the snake, deer, &c. Each tribe forms a little community within the nation; and as the nation has its peculiar symbol, so has each tribe the particular badge from which it is denominated: and a Sachem of each tribe being a necessary party to a fair conveyance, such Sachem affixes the mark of his tribe thereto, like the public seal of a corporation. With respect to the deed of 1726, of which you sent me the signatures, the transaction was in some measure of a partial nature. All the nations of the confederacy did not subscribe it; and those chiefs who did, neglected to pay due regard to their proper symbols; but signed agreeably to fancy, of which I have seen other instances. The manner I have mentioned is the most authentic, and conformable to their original practice.

As to the information, which, you observe, I formerly transmitted to the Governor of New York,

York, concerning the belt and 15 bloody sticks sent by the Missifagees, the like is very common; and they use these sticks, as well to express the alliance of castles, as the number of individuals in a party. The sticks are generally about 6 inches in length, very slender, and painted red if the subject be war. Their belts are mostly black wampum, painted red when they denote war. They describe castles sometimes upon them, by square figures of white wampum; and in alliances, human figures holding a chain, which is their emblem of friendship, and each figure represents a nation. An axe is also sometimes described, and always imports war: the taking it up, being a declaration of war; and the burying it, a token of peace.

With respect to your questions concerning the chief magistrate, or Sachem, and how he acquires his authority, &c.; I am to acquaint you, that there is, in every nation, a Sachem, or chief; who appears to have some authority over the rest, and it is greatest amongst the most distant nations. But in most of those bordering on our settlements, his authority is scarcely discernible, he seldom assuming any power before his people. And indeed this humility is judged the best policy; for, wanting coercive power, their commands would perhaps occasion assassination, which sometimes happens.

The Sachems of each tribe are usually chosen in a public assembly of the chiefs and warriors, whenever a vacancy happens by death or otherwise; they are generally chosen for their sense
and

and bravery from among the oldest warriors, and approved of by all the tribe; on which they are saluted Sachems. There are, however, several exceptions; for some families have a kind of inheritance in the office, and are called to this station in their infancy.

The chief Sachem, by some called the King, is so either by inheritance or by a kind of tacit consent, the consequence of his superior abilities and influence. The duration of his authority depends much on his own wisdom, the number and consequence of his relations, and the strength of his particular tribe. But even in those cases where it descends, should the successor appear unequal to the task, some other Sachem is sure to possess himself of the power and the duties of the office. I should have observed, that military services are the chief recommendations to this rank. And it appears pretty clearly, that heretofore the chief of a nation had, in some small degree, the authority of a sovereign. This is now the fact among the most remote Indians. But as, since the introduction of fire arms, they no longer fight in close bodies, but every man is his own General; I am inclined to think this has contributed to lessen the power of a chief. This chief of a whole nation has the custody of the belts of wampum, &c. which are as records of public transactions: he prompts the speakers at all treaties, and proposes affairs of consequence. The chief Sachems form the grand council; and those of each tribe often deliberate apart on the affairs of their particular tribes. All their deliberations

are conducted with extraordinary regularity and decorum. They never interrupt him who is speaking; nor use harsh language, whatever may be their thoughts.

The chiefs assume most authority in the field: but this must be done, even there, with great caution; as a head warrior thinks himself of most consequence in that place.

The Indians believe in, and are much afraid of, witchcraft: those suspected of it are therefore often punished with death. Several nations are equally severe on those guilty of theft, (a crime indeed uncommon among them): but in cases of murder, the relations are left to take what revenge they please. In general, they are unwilling to inflict capital punishments, as these defeat their grand political object, which is, to increase their numbers by all possible means.

On their hunts, as upon all other occasions, they are strict observers of *meum* and *tuum*, and this from principle, holding theft in contempt; so that they are rarely guilty of it, though tempted by articles of much value. Neither do the strong attempt to seize the prey of the weak; and I must do them the justice to say, that, unless heated by liquor or inflamed by revenge, their ideas of right and wrong, and their practices in consequence of them, would, if more known, do them much honor. It is true, that, having been often deceived by us in the purchase of lands, in trade, and other transactions, many of them begin now to act the same part. But this reflects most on those who set them the example.

As to your remark on their apparent repugnance to civilization, I must observe, that this is not owing to any viciousness of their nature, or want of capacity; as they have a strong genius for arts, and uncommon patience. I believe they are put to the English schools too late, and sent back too soon to their people, whose political maxim, Spartan-like, is to discountenance all pursuits but war, holding all other knowledge as unworthy the dignity of man, and tending to enervate and divert them from that warfare on which they conceive their liberty and happiness depend. These sentiments constantly instilled into the minds of youth, and illustrated by examples drawn from the contemptible state of the domesticated tribes, leave lasting impressions; and can hardly be defeated by an ordinary school education.

I wish my present leisure would allow me to give you as many specimens of their language as would shew, that (though not very wordy) it is extremely emphatical; and their style adorned with noble images, strong metaphors, and equal in allegory to many of the Eastern nations. The article is contained in the noun, by varying the termination; and the adjective is combined into one word. Thus of *Echin*, a man, and *Gowana*, great, is formed *Echingowana*, a great man. *Caghyungbarw* is a creek, *Caghyungba* a river, *Caghyungbaowana* a great river; *Caghyungbeeo* a fine river. *Haga* the inhabitants of any place, and *Tierham* the morning; so, if they speak of Eastern people, they say *Tierhans-aga*, or people of the morning. *Eso* is expressive of a great quantity,

and *Esojee* is the superlative. The words *Goronta* and *Golota*, which you mention, are not of the six nations, but a Southern language. It is curious to observe, that they have various modes of speech and phrases peculiar to each age and sex, which they strictly observe. For instance, a man says, when he is hungry, *Cadagcariax*, which is expressive both of his want and of the animal food he requires to supply it; whilst a child says, in the same circumstances, *Cautsere*, that is, I require spoon-meat.

There is so remarkable a difference in the language of the six nations from all others, as affords ground for enquiring into their distinct origin. The nations North of the St. Lawrence, those West of the great lakes with the few who inhabit the sea-coasts of New England, and those again who live about the Ohio, notwithstanding the respective distances between them, speak a language radically the same, and can in general communicate their wants to one another; while the six nations, who live in the midst of them, are incapable of conveying a single idea to their neighbours, nor can they pronounce a word of their language with correctness. The letters *M* and *P*, which occur frequently in the other languages, are not in theirs; nor can they pronounce them but with the utmost difficulty. There is indeed some difference of dialect among the six nations themselves; but this is little more than what is found in all the European states.

Received January 15, 1772.

XVIII. *An Account of some curious Fishes, sent from Hudson's Bay; by Mr. John Reinhold Forster, F. R. S. in a Letter to Thomas Pennant, Esq; F. R. S.*

DEAR SIR,

Read Jan. 28, 1773. **T**HE Governor and Committee of the Hudson's Bay Company presented the Royal Society with a choice collection of skins of quadrupeds, many fine birds, and some fish, collected by their servants at the several ports in Hudson's Bay; the Committee of the Royal Society, for examining and describing these curiosities, did me the honour to refer them to me for examination. I wish the following observations on the fish, which I take the liberty to address to you, as a lover of Natural History, and my remarks on the birds and quadrupeds, may convey such informations concerning the zoology of North America, in the study of which you have made such vast progress; and so particularize the animals of the *Arctic* part of that vast continent, that nothing further may prevent
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your

your favouring the public with the result of your studies on that subject.

The four kinds of Hudson's Bay fish are the *Sturgeon*, the *Burbot*, the *Gwiniad*, and a new fish called the *Sucker* at Hudson's Bay.

The *Sturgeon* was about fourteen inches long, and therefore seems to be a young fish; as it is likewise observed in the list, written by the Gentleman who sent this fish from York Fort.

DESCRIPTION.

Its nose is very long and slender, terminating in a point; the eyes are small; under the projecting snout, before the mouth, are four beards or *cirri*, placed nearly in the same line, and not by pairs, as in some other species of *Sturgeon*. The mouth is beneath, nearly opposite the eyes, toothless, cartilaginous, semilunar when in its natural position, but round when open; on each side are two nostrils. The whole head is depressed, and very nearly quadrangular; the whole body pentagonal, and tapering towards the tail; the whole skin tough, covered with five rows of uncinated scales; the dorsal series consists of fourteen large roundish scales, and a single one behind the dorsal fin; each of the lateral rows has 35 oblique scales; in the two ventral rows are nine roundish strong scales between the pectoral and ventral fins; one scale is behind the vent, and still another behind the anal fin.

The fish, according to this description, seems to come the nearest to that species of *Sturgeon* which
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I described in the Philos. Transactions, Vol. LVII. in my *Specimen Historiæ Naturalis Volgensis*, N° 10. under the name of *Acipenser Ruthenus major, rostro elongato acuminato, paululum supino*, and which the Russians call *Sevruga*. Kramer, in his *Elenchus Vegetabilium & Animalium Austriæ*, p. 383. is the only writer that I know who takes notice of this species; he calls it *Acipenser rostro acuto, corpore tuberculis spinosis aspero*: the inhabitants of Austria call it *Shirk*, a name they have no doubt borrowed from the Slavonian name *Sevruga*. The famous painter and traveller Cornelys de Bruyn mentions this kind of fish, but in so superficial a manner, that one plainly sees he was little, if at all, used to discussions in points of Natural History. He says, * “ the *Sterlet* is the “ best fish in Russia; there are two species of it; “ but, upon the whole, it is nearly related to the “ Sturgeon. The *Severukas* differ in nothing from “ the Sturgeon, which the Russians call *Assetrina*. “ The Caviar is made from the *Beluga*, the *Assetrina*, and the *Severuka*.” Had de Bruyn examined the *Sevruga*, he would certainly have found it materially different from the *Osetr* or *Assetrina*, i. e. the common blunt-nosed Sturgeon of Germany and the Baltick. I suppose the English Sturgeon, from your own description †, and the drawing in the British Zoology, illustrated by plates, tab. LXXXIX. to be the same with this kind from Hudson’s Bay, and with the *Sevruga* of the Russians, and the *Shirk* of the Austrians. The true Sturgeon, which gave

* De Bruyn’s Voyage, Tom. I. &c. Amsterd. fol. p. 93.

† Br. Zool. octavo, Vol. III. p. 99.

the name to the whole genus *, I think to be an unknown fish in England. The species of Sturgeons are more numerous than one is at first aware of; and it would therefore be of some utility, that persons, who have an opportunity of examining all the various kinds at Vienna, and in Russia, might do it with more care than has hitherto been done. Some of the sorts which I have seen, I have so described that they may be known again; some I did not see, and gave their characters from books, and from the reports of such persons as had examined them. Mr. Klein, a very ingenious naturalist, has enumerated ten Sturgeons, in his 4th *Missus Piscium*, p. 11—16. and Count Marfigli, in his splendid work about the Danube, Tom. IV. gives the names of at least six Sturgeons, but the characters are not sufficiently settled in both these works. Klein saw but two kinds of Sturgeons, and a third in spirits; and Count Marfigli was not enough of a naturalist to give adequate descriptions of these fish. Therefore it is certain that a careful examination and accurate account of the several species of Sturgeons would greatly illustrate the Natural History of this genus.

The second of the Hudson's Bay fish, is called, by the wild natives of that country, *Martby*, and is nothing else than our common Burbot, *Gadus Lotæ*, Linn. only vastly superior in size. The descriptions

* The Germans call this fish *Stour*, from the old Teutonic word *Star* or *Stuhr*, which signifies *great*, as this fish grows to a very large size. Thus likewise the Scotch call the Tunny, Mac-krel *Sture*. Vide Mr. Pennant's Tour in Scotland, p. 192.

you have given of this fish, in the British Zoology, is entirely corresponding with this specimen, so that it would be superfluous to presume to make any additions to it. I must, however, observe, that, after a most minute examination, I could find no more than six branchiostegous rays in the two specimens from Hudson's Bay, of which you mention seven in the English Burbot, and Artedi as many in his specimens. This great naturalist seems likewise to be right, when he observes that the *cirri*, or beards on the end of the nose, are the valves to one of the nostrils; for I found that these beards, on their under-side, opened into a hole, corresponding with the lower nostril. Mr. Andrew Graham, the collector of the Natural History specimens at Severn River in Hudson's Bay, observes, that these fish constantly swim close to the ground, and are extremely voracious; for he represents them as not content with devouring every fish * they can overcome, but likewise feeding on putrefying deer, or other carrion that comes in their way; even stones are sometimes swallowed to satisfy their insatiable appetite, of which Mr. Graham was himself a witness, having taken a stone of a pound weight out of the stomach of this fish. The pike is often obliged to fall a victim, together with the trout, *Tickomeg*, and others, to this rapacious fish. After sunset, it is caught by a night-hook. It does not masticate its food before deglutition. Its roe and liver are reckoned a delicacy, when fresh caught; but they turn rancid and

* This too is the fish that makes such havoc in the Lake of Geneva. P.

oily in a few days, though kept frozen solid all the time. At Hudson's Bay this fish is thought to be dry and insipid ; its weight is from one to eight pounds.

The third species of fish, from this cold climate, is by the natives called *Tickomeg*, and is our *Gwiniad* or *Salmo Lavaretus*, Linn. ; only the size is somewhat bigger, for the greatest specimen sent over measures 18 inches from the head to the tip of the tail, is $4\frac{1}{2}$ inches deep, and not above an inch and $\frac{1}{4}$ thick. This fish differs in no circumstance from our *Gwiniad*, but the length. You mentioned in your *British Zoology* (Vol. III. p. 269.) a *Ferra* or *Gwiniad* from Switzerland 15 inches long, as an uncommon size * ; the Hudson's Bay fish, as I have before observed, is 18 inches long, and $4\frac{1}{2}$ inches its greatest depth. The great abundance of food, and the small number of inhabitants, who let the fish grow up undisturbed, are perhaps the causes of their uncommon bigness. They weigh from $1\frac{1}{2}$ pound to 3 pounds, says Mr. Graham ; but, I am sure, the fish I examined must, when fresh, have weighed more. These fish abound in the River Severn in Hudson's Bay, from its origin in the great lakes to its mouth, where it empties itself into the bay. The natives catch five or six hundred a day, by means of weirs which they contrive in the river : they will not take bait, and are poor at the breaking of the ice in the river. In the middle of the summer, after a gale of wind,

* However, the *Gwiniads* of Lapland, a similar climate to that of the Hudson's Bay, are vastly large. *Brit. Zool.* III, 267. note.

they are often found thrown up into the marshes, and on the shoals, where they remain at the recess of the water and abating of the wind, and serve as food to numbers of crows. The inhabitants of Hudson's Bay think this fish very sweet, and good to eat, contrary to the opinion of many Europeans.

The fourth and last fish brought from Hudson's Bay is there called a *Sucker*, because it lives by suction, according to Mr. Graham's account, who likewise says, that there are two varieties of this fish, both of a whitish colour, but one distinguished by a mixture of beautiful red. In the smallest of two specimens brought over, a broad stripe of red could be observed all along the *linea lateralis*. They are very numerous in the creeks and rivers, and troublesome in overburdening the nets. They are not deemed a palatable food, being very soft, and full of small bones. They weigh from one half to $2\frac{1}{2}$ pounds.

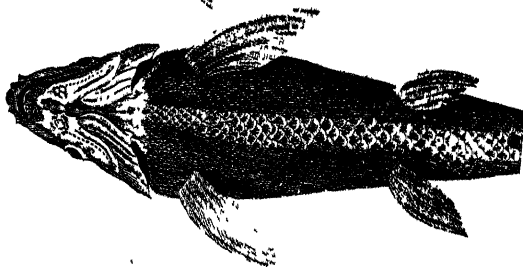
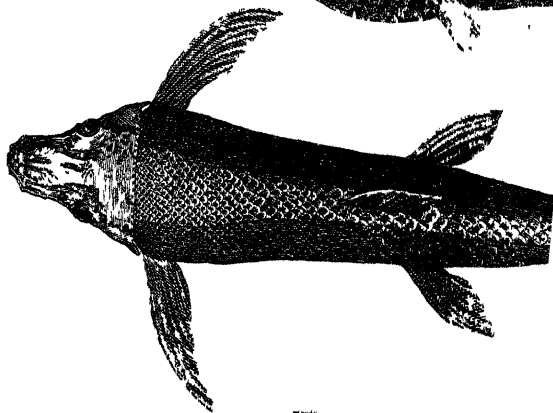
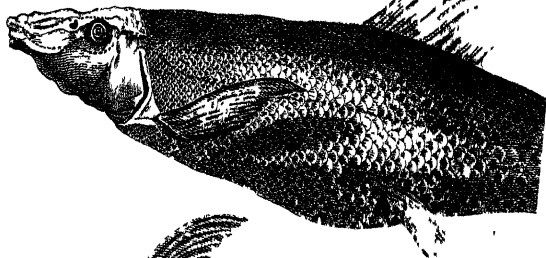
The above is literally what Mr. Graham says of this fish, and all that is known of its natural history. Examining it carefully, I found it was a new species of the genus of *Cyprinus*, or *Carp*.

The head is broader than the body, gradually decreasing towards the nose, full of elevations and tubercles, nearly quadrangular, and not scaly. The mouth is quite under the head, as in the *Loricariæ*, when shut, semilunar; when open, round; not far from the extremity of the snout, and included in small round lips. To the under-lip is fixed a bilobated, beard-like, papillose caruncula; it has no teeth. The

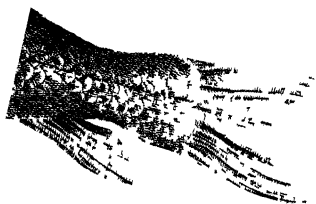
eyes are large, but the colour of the iris could not be determined. The number of the branchiostegous rays is three. The body is flat, tapering towards the tail, and scaly. The greater specimen measures very near 15 inches from the nose to the extremity of the tail; next to the head it is nearly two inches thick, about the dorsal fin $1\frac{1}{4}$ inch; its greatest depth before the ventral fins is $2\frac{4}{7}$ inches. On the snout are about five round prominent tubercles; two nostrils are found on each side, the biggest next before the eye is kidney-shaped. The covers of the gills are double, and divided; the head has several sutures; over each eye, in a cavity, are two longitudinal ones, joined opposite the nostrils by a still shorter transverse one; on the covers of the gills are two, on each side one, beginning near the lobes of the caruncula of the under-lip, and going up arched towards the eye. Near the extremity of the snout begins on each side a longitudinal one; it passes under the eye, and mounts in a curvature behind it, then it goes on straight to the end of the head, where it again gets downwards, and joins the lateral line. Where the head joins to the body, these two sutures are connected by a transversal one, which, as it were, separates the head from the body. The lateral line at first descends from the head, but then runs on straight, rather nearer the back than the body, to the beginning of the tail. The scales are small near the head and back, increasing in size towards the middle and tail, close to which they are again smaller. The dorsal fin is placed somewhat behind the equilibrium of the fish, rhomboidal, and consisting of twelve strong branched rays. The pectoral fins are lanceo-

Cyprinus Catostomus.

Sh.



W. Lucher



lanceolated, fixed under the covers of the gills, and have 17 rays. The ventral fins have 10 or 11 rays, and are placed in the middle of the belly, and under the dorsal fin. The anal fin consists of eight branched strong rays. The tail is somewhat forked or concave, and consists of seventeen rays.

I have been as circumstantial as possible in describing this new species, and join here, together with a drawing [See TAB. VI.], a Latin scientific description of the same.

I am,

With the truest regard,

DEAR SIR,

Your most obedient,

humble servants,

Jn^o Reinhold Forster.

N^o 2. Somerset Stable-yard, Strand,
January 12, 1772.

Κατω
 infra } CYPRINUS Catostomus.
 Στομαος.

CYPRINUS pinna ani radiis VIII. labio imo caruncula bilobata papillosa, cauda bifida. Pinnæ D. 12. P. 17. V. 10.—II. A. 8. C. 17.
 Habitat in Sinus Hudsonis fluminibus copiose, fugendo pascitur. Anglis, the *Sucker*.

DESC. *Caput* subtetragonum, versus apicem sensim attenuatum, obtusiusculum, corpore fere crassius, & minus latum.
Tubercula globosa, confertiora in apice rostri, circiter quinque; carinata & acuminata, in vertice sparsa.
Foramina (five nares) gemina, quorum alterum minus, alterum oculis proximum, majus, reniforme.
Oculi magni, ad marginem superficiei verticalis capitis siti, fere in medio inter apicem & basin. *Irides*
Opercula branchiarum magna, nuda; at sub oculis opercula spuria, primo intuitu pro radiis membranæ branchioftegæ facile sumenda.
Suturæ in capite plures catenulatæ; una utrinque brevis, supra oculos, naresque, nec basin nec apicem capitis attingens, è regione narium juncta per futuram transversalem brevissimam; secunda utrinque incipiens ad angulum lorum carunculæ,

carunculæ, imo labio adnata, in operculo spurio recurvatur, & prope oculos definit: tertia utrinque incipiens prope rostri apicem, linea recta sub oculis ducta, dein curvatur & ascendit versus verticem; ibi rursus curvatur & jungitur lineæ laterali, pone aperturam branchialem descendenti: connectuntur hæ duæ lineæ laterales futura transversa, quæ caput à reliquo corpore distinguit.

Membrana Branchiostega, radiis tribus brevibus, validis.

Rictus inferus, lunulatus, seu femiorbicularis, labiis inclusus tenuibus, superiore (ore scilicet clauso) concavo, inferiore convexo.

Caruncula lata, labio inferiori adnata, crassiuscula, carnosæ, papillis tectæ, oris angulos ambiens, medio in lobos binos profunde divisa.

CORPUS lateribus compressiusculum, at versus abdomen magis compressum, cuneiforme, capite ad caudam sensim attenuatum, tectum squamis minoribus, ovatis, striatis versus caput minimis, pallide argenteis, in quibusdam circa lineam lateralem aureo-rubris.

Linea lateralis recta, dorso parallela, ad caput super aperturam branchialem ascendens.

Anus parvus, caudæ multo proprior quam capiti.

Pinna dorsi pone æquilibrium nonnihil posita, rhomboidalis, radiis validis, ramosis duodecim.

Pinnae pectorales lanceolatæ, infra opercula sitæ radiis 17 longitudine partem quartam totius piscis (exclusis capite & caudâ) æquant.

Pinnae ventrales radiis 10 vel 11 oblongæ, in medio ventris, sub pinna dorsali posita, pinna pectorali dimidio breviores.

Pinna ani caudæ propinqua, longitudine fere pinnae pectoralis, radiis octo validis, ramosis.

Cauda leviter bifurca, pinnam pectoralem longitudine & numero radiorum æquans.

Longitudo totius piscis unciarum 15 pedis Anglicani.

Latitudo unciarum circiter 3 ante pinnas ventrales.

Craffitiæ corporis prope caput unciarum fere 2, ante pinnam dorsalem uncie & quadrantis.

P. S. Besides the above-mentioned fish, the servants of the Hudson's Bay Company have likewise sent over from thence the common *River Crayfish* (*Cancer Astacus*, *Linn.*), which, in every particular, corresponds with the English one.

Received November 12, 1777:

XIX. *Experiments upon the different Kinds of Marle found in Staffordshire, by William Withering, M. D. Communicated by Charles Morton, M. D. Sec. R. S.*

Read Feb. 4, 1773.

Description.	Quantity of calcarious earth in half a dram, as separated by the nitrous acid, and precipitated by mild fixed alkaly.		What was left after the foregoing separation, was no longer acted upon by the nitrous acid; but being calined, weighed		The calined marles being put into lime water, produced	
	Grains.	Mixed with water, became	When burnt	Grains. Left grains.	Burnt to	
1. Red and blue intermixed, in small friable lumps.	1	Uniform and plastic.	A hard red brick.	52	Red brick.	No effect.
2. Red, in small friable lumps.	0½	Uniform and plastic.	A hard red brick.	53	Red brick.	No effect.
3. Grey, in large hard lumps.	5	Plastic, but a little gritty.	A soft yellowish brick.	49	Soft yellow brick.	Weak lime water.
4. Red, hard, compact.	3	Uniform and plastic.	A hard red brick.	50	Red brick.	No effect.
5. Red, with grey spots, in large hard lumps, scarcely to be broken with a hammer.	8½	Plastic.	A soft pale red brick.	48	Hard grey stone.	Lime water.
6. Light grey, like a grit stone.	8	Gritty, no union.	No union.	51	Soft and flaky.	Lime water.
7. Brown, friable, in large lumps.	18	No union.	A little cohesion.	46	Soft stone.	Lime water.
8. Red, in large friable lumps.	14	Plastic, but a little gritty.	A soft red brick.	48	Soft stone.	Strong lime water.
9. Brownish white, very hard, like calcareous incrustations.	16	No union, gritty.	No union.	43	Soft stone.	Strong lime water.
10. Lead colour, friable, flaky.	14½	No union, gritty.	No union.	48	Soft stone.	Strong lime water.
11. Brown grey, very hard, in irregular lumps.	16	No union, gritty.	No union.	40	Soft stone.	Strong lime water.
12. Lead colour, in powder and in small hard lumps.	20½	Uniform and plastic.	A soft whitish brick.	29	Powdery.	Strong lime water.

Half a dram of the marles being put into similar glass cups, two drams of nitrous acid being added to each glass, they all effervesced; N^o 1 and 2 the least, N^o 12 the most. The effervescence having ceased, and six drams of rain water being added to each glass, the liquors were all filtered, and after filtration, changed violet paper to a red colour. To the filtered colours was gradually added mild fixed alkali, sufficient to saturate the acid, and precipitate all the earth it had dissolved. The precipitated earth being washed in rain water, till free from all saline matter, weighed, when dry, as in column the third. Column the fourth shews that, after the separation of the calcarious earth, there remained in N^o 1, 2, 4, a red clay; in N^o 12 a white clay; in N^o 8 a red clay, and a portion of sand; in N^o 3 a whitish clay, with a portion of sand; in N^o 6, 9, 10, 11, pure sand; and in N^o 7, sand, with a small portion of clay. These residuums were all washed with rain water before they were burnt. The precipitated powders being mixed together, 82 grains thereof put into a crucible, and calcined with a strong hear, lost 35 grains in weight. Rain water was poured upon the calx; the next morning there was a pellicle upon the surface of the water; it tasted strongly of lime, and let fall a calcarious earth, upon the addition of mild fixed alkaly. The marles were kept for some weeks in a dry place before they were made use of. They were all got out of marle pits in the neighbourhood of Stafford, except N^o 12, which is found near the Duke of Bridgewater's canal, in a powdery form, and when mixed with one fourth part of clay is burnt to quick lime. All the above marles crack and fall to pieces, when exposed to the weather.

The foregoing experiments were undertaken with a view to ascertain how far it would be advisable to attempt burning the marles of this country into quicklime, for the purposes of agriculture; they may likewise furnish us with some useful hints relative to the kind of marles proper to be used upon different kinds of lands. Perhaps the calcarious earth united with clay, as in N^o 1, 2, 4, &c. may be the best for light sandy soil; and N^o 6, 9, 10, 11, where the calcarious earth is united with sand, the most digible where the land is already stiff, and abounding with clay. How far the different quantities of fixable air, or other volatile parts, contained in each of the marles, as shewn by column the fifth, will influence their preference in agriculture, must be left to the experience of the farmer to determine.

Received Nov. 12, 1772.

XX. *A Letter from Patrick Brydone, Esq; to Sir John Pringle, Bart. Pres. R. S. containing an Account of a fiery Meteor, seen on the 10th of February last; and also of some new electrical Experiments. Dated Eccles, (in the Shire of Berwick) 7 July, 1772.*

S I R,

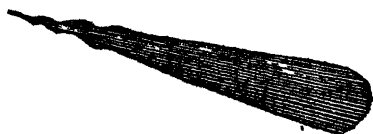
Read Feb. 4, 1773. **A**BOUT fourteen years ago, I took the liberty of communicating to you some electrical experiments; which, you thinking worthy of the attention of the Royal Society, were pleased to lay before that respectable body; and those gentlemen did me the honour of publishing them in the first and second part of the fiftieth Volumes of the Philosophical Transactions. Since that time, I have often been engaged in operations of the same kind, some of which, particularly the experiments with the electrical kite, I thought to have troubled you with; but as these were made in foreign countries, and as I was informed that something of the same kind had been done in England, I suspected that you might al-

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ready have been acquainted with them; and this consideration prevented me. However, last winter, on my return to this kingdom, I observed some facts in electricity, which I flatter myself you will not think undeserving of your notice. What led me to make these experiments, was the strong electrical appearances that the air exhibited during the last great frost, and the observation of several meteors, and other phænomena, that possibly depend on electrical causes. One of these meteors was so remarkable, that I must beg leave to give you some short account of it.

On Monday the 10th of February last, exactly at seven in the evening, as I was riding through Tweedmouth, a village at the south end of Berwick-bridge, I observed that the atmosphere was suddenly illuminated in a very extraordinary manner. The light of the moon, which was about half full, seemed to be extinguished by the blaze; and I saw my shadow projected on the ground, and almost as distinct, and well-defined, as in sunshine. I turned round to see from whence the light proceeded, when I beheld a long, bright flame, moving almost horizontally along the heavens. It was of a conical form, and from the base to the apex could not be less than six or seven degrees; its height, when I first observed it, seemed to be about fifty degrees; but it descended gently, and appeared to burst about five or six degrees lower. Its course was from north-west to south-east, and seemed to have an inclination to the horizon; but this might be only a deception. The base of the cone was rounded like a sphere;
and

and apparently of about one third of the diameter of the moon at her greatest height; but its light was brighter than that of the planet Venus, and in colour resembled the flame of burning camphire. Near the end of the tail there was a kind of waving motion, which with the whole appearance, I have endeavoured to represent by this figure —



In about ten or twelve seconds it seemed to burst, dividing into a number of small luminous bodies, like the stars in a sky-rocket, which immediately disappeared.

As I had formerly observed explosions from meteors of this kind, I had presence of mind to pull out my watch (which has a second hand) to measure the exact time the report should take in reaching me. I waited for upwards of four minutes, which in my state of expectation appeared a much longer time; when, despairing of any report, I rode on, but had not got to the middle of the bridge, when I was stunned by a loud and heavy explosion, resembling the discharge of a large mortar, at no great distance, and followed by a kind of rumbling noise, like that of thunder. I examined my watch, and found, that the sound had taken five minutes, and about seven seconds, to reach me; which, according to the common computation of 1142 feet in a second, amounts to the distance of at least 66 miles. It did not occur to me to measure the duration of the light,

which probably did not exceed ten or twelve seconds; and during this short period, the length of the path, the meteor seemed to describe, could not be less than 30 degrees. I expected to have seen some account of this phenomenon from Newcastle, as, by its direction and distance, I imagined it had burst pretty near to the zenith of that city; but I have found no notice taken of it in the news-papers there. About a week after, I mentioned what I had seen to Sir John Paterfon, of Eccles, who told me he was at that time on the road, betwixt Greenlaw and his own house, in company with Mr. Thomas Cockburn, of Edinburgh; and, as they were riding to the south, they observed the meteor from its first appearance, which was about three or four seconds sooner than I had time to turn about and view it; and this, perhaps, is the reason that it appeared so much higher to them than it did to me. These gentlemen observed, that when it first became luminous, it was almost vertical, but went off descending to the south-east, and had in other respects the appearance I have described. They added, that some considerable time after the light disappeared, they heard a great report, which they took for a clap of thunder; for the interval was so long, that they did not imagine this sound had any connection with what they had seen.

Now, as these gentlemen were at least 20 miles to the west of the spot where I made my observation, and as the appearance and height of this meteor seems to have been nearly the same to them as to me, it is probable that it was at a very
great

great distance from the earth, and much beyond the limits that have been assigned to our atmosphere. The smaller meteors, which we call falling stars, I have frequently observed from the mountain of St. Bernard, one of the high Alps; and last year I had the good fortune to see several of them from the highest region of Mount Etna; an elevation still more considerable, and probably the greatest accessible one in Europe, and they always appeared as high, as when seen from the lowest grounds; so that probably the height of two or three miles, bears but a small proportion to the common altitude of these bodies.

From their frequent appearance, during the last frost, I was inclined to believe, that the air was then in a very favourable state for electrical purposes; but not being provided with a common machine, I bethought me of a whimsical one to supply the want of it. The back of a cat, it is well known, often exhibits strong marks of electricity; being, therefore, desirous to try what effect this might produce, when made use of instead of the glass globe, I cut a quantity of harpsichord-wire into short pieces, of five or six inches, and tying them together at one end, made the other diverge like the hair of a brush. I took a large metal pestle of a mortar for my conductor, to the end of which I fixed the brush of wire; and insulated the whole, by placing it on a couple of wine-glasses. I then took a cat on my knee, and bringing her back under the wires, I began to stroak it gently. The animal continued in good humour for a few minutes, and I had the satis-

faction to see that the conductor was so much charged, that it emitted sparks of a considerable force, and attracted strongly such light bodies as were brought near it; but the cat at last becoming uneasy, threatened to put an end to our experiment. The passage of the electrical fire, from the hair of her back to the small wires, occasioned, it seems, a disagreeable sensation, which she could not bear; so that turning about her head to defend her back, the tip of her ear happening to touch the conductor, and a large spark coming from it, she sprung away in a fright, and would not allow me to come near her more. However, after a long interval, the animal seeming to have forgotten her adventure, a young lady in company, less obnoxious to her than I was, undertook to manage her. Having first covered the back of this lady's hand with a piece of dry silk, that none of the electric fire communicated to the wires might be lost, she then began to stroke the cat as I had done, and the conductor soon after appeared fully charged: we drew large sparks from it, and if the animal would have continued quiet, I have no doubt that we should have shewn many of the common experiments in electricity; but she soon became so outrageous, that we were glad to put an end to our operations, without any hopes of being able to repeat them, at least with the same instrument. In this dilemma I recollected, that a lady had told me, that on combing her hair, in frosty weather, she had often been sensible of a little crackling noise; and in the dark had sometimes observed small sparks of fire to issue from it. I proposed, therefore,

therefore, that one of the young ladies would suffer the experiment to be made upon her head, which she agreed to. The conductor was then insulated as before, and the lady having placed herself so, that the back part of her head almost touched the brush of wire, I desired her sister to stand behind her, on a cake of bees-wax; who, as soon as she began to comb the hair of the former, the conductor emitted sparks still of a larger size than those we had hitherto seen. The hair was extremely electric, and when the room was darkened, we could perceive the fire pass from it along the small wires to the conductor. The young lady who was on the wax, was not a little surprized to find, that the moment she began to comb her sister's hair, her own body became electric, darting out sparks of fire against every substance that approached her. We found, however, that these sparks were not strong enough to fire spirits. I then coated a small phial, and soon charged it from the conductor; but afterwards I did it more compleatly from the hair itself in the following manner. I fixed a brush of small wires to the large one that went through the cork of the phial; and taking the phial in my hand, I followed every motion of the comb with the brush of wires; and, in the dark, could observe the fire pass by these wires into the bottle. In a few minutes I found it was highly charged; when taking a spoonful of warm spirits in my left hand, and with my right, which grasped the phial, bringing the hook of the great wire near the surface of the spirits, a large spark darted from it,

gave

gave me a smart shock, and at the same time set the spirits on fire.

The day following, we wanted to repeat our experiments; but as the weather was hazy, and the frost had greatly abated, they did not so well answer. However, from making them on several heads, we found that the stronger the hair, the greater was the effect; whereas, soft flaxen hair produced little or no fire at all.

It may not be improper to mention, that these experiments were made in a warm, dry room, before a good fire, and at a time when the thermometer, in the open air, was at six or seven degrees below the point of congelation. The hair, which succeeded best, was perfectly dry, and no powder or pomatum had been used on it for some months before.

I have the honour to be,

S I R,

Your most obedient,

and most humble servant.

Patrick Brydone.



XXI. *Some Account of a Fossil lately found near Christ-Church, in Hampshire; in a Letter to Dr. Maty, Sec. R. S. from the Hon. Daines Barrington, Vice-Pref. R. S.*

DEAR SIR,

Read Feb. 4, 1773. I Send herewith a fossil which was found in a cliff, near Christ-Church, in Hampshire, by the Rev. Mr. Howel, of Pool, and which Mr. Gough, F. A. S. desires may be shewn to the Royal Society, if it should be thought worthy their attention. [See Tab. VII.]

I am very little versed in this branch of natural history; but as I am the unworthy channel of communication, I thought it my duty to procure the best information I was able, with regard to this very singular specimen.

The shining divisions, upon the surface of the stone, seem to be the scales of a fish, which I should conceive to be the *Acus maxima squamosa*, engraved in Willoughby's History of Fish, Tab. p. 8. and described by Ray, in his Synopsis Piscium, p. 109.

It appears by the catalogue of English fossils, in the collection of Dr. Woodward, that a still larger specimen of the same sort was found in Stansfield quarry, near Woodstock, though Dr. Woodward could only procure a single scale. V. II.

P. 53. c. 24.

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A a

Single

Single scales from the same quarry, are also to be seen in the noble collection of fossils, given by Mr. Brander, F. R. S. to the British Museum.

Though this fish, therefore, is a stranger to our seas, yet its exuviae are by no means so to our cliffs and quarries.

I am,

Dear SIR,

Your most faithful

humble servant,

Daines Barrington.

P. S. Mr. Hunter, F. R. S. having seen the fossil at Crane-Court, happened to dissect a beaver's tail very soon afterwards, which he shewed me, as bearing a strong resemblance to the scaly divisions in this specimen; I cannot, however, but still think that the form of the scales in the *Acus maxima squamosa* of Willoughby is still nearer to it, than of those in a beaver's tail.

XXII. *A Description of a rare American Plant of the Brownææ Kind; with some Remarks on this Genus. By Mr. Peter Jonas Bergius, F. R. S.*

Read Feb. 11, 1773. **A**S the *Leucandendra*, *Bruniæ*, *Diosmæ*, *Phylicæ*, *Hermannicæ*, &c. are peculiar to Africa, so are likewise the *Varroniæ*, *Ehreticæ*, *Samydæ*, *Malpighiæ*, *Caëti*, *Brownææ*, &c. peculiar to America, not having been found in any other country: at present I will confine myself to the last mentioned kind.

Mr. Jacquin, during his botanical travels in America, founded this genus, in memory of Dr. Patrick Browne, the celebrated English botanist; but Jacquin found only one species of this genus; neither was Sir Ch. Linné hitherto acquainted with any more.

I have now specimens of a new species of this kind, which I received from Mr. Pihl, who gathered it in Portobello in America, which will afford an opportunity of exhibiting the whole genus of the *Brownææ*, and the specific differences of it.

If we compare Mr. Jacquin's description of his species with mine, we see how carefully nature has observed the same order and position of the essential

parts in both ; a circumstance common to all natural genera.

I don't know whether this plant will vegetate and thrive in our stoves or green-houses ; if it does, I am convinced it will make a beautiful appearance with its assemblage of purple or blood-red flowers.

The drawings which I herewith send [TAB. VIII. and IX.], shew a stalk with a flower, and one without, so that the receptacle is bare.

BROWNÆÆ Genus.

1. BROWNÆA (*coccinea*) floribus disjunctis umbellatis.

Brownæa coccinea. Linn. Spec. Plant. 958.

Jacquin. Hist. Stirp. Americ. 194. * Tab. 121.

Habitat in rupestribus sylvaticis Zaucæ ad finum Venezuelæ Americes.

Descr. apud Jacquinum, loco citato.

2. BROWNÆA (*Rosa de monte*), floribus aggregato-capitatis sessilibus, staminibus longissimis.

Hermesias. Loefling. Itin. p. 278. *

Habitat in montosis Terræ Firmæ, Portobello.

DESCR. *Caulis* arboreus.

Rami torulosi, cortice cinereo.

Ramuli (seu petioli communes) subalterni, teretes, glabri, basi geniculo suberoso-rugoso, patentes.

Folia coriacea, spithamæa, opposita, ovato-oblonga, integerrima, longius acuta, utrinque glabra, nervis alternis obsoletis, breviter petiolata ; inferiora sensim minora ; infima ovata, basi subcordata.

Petiolelli

Petiolelli breves, crassi, rugosi.

Flores intra calycem communem aggregati in capitulum f. fasciculum subrotundum, speciosissimum, magnitudine pugni.

Fasciculi solitarii, alterni, distantes, sessiles, subaxillares.

Calyx communis imbricatus : foliolis f. bracteis ovatis, acutiusculis, submembranaceis, concavis, laxiusculis, glabris, bipollicaribus, rubicundis ; singulis includentibus singulos vel etiam 2 aut 3 flores ; deciduis ; exterioribus rotundatis ; interioribus minoribus, sensim linearibus.

Perianthium proprium cylindricum, tubulatum, superne paululum ampliatum, rubicundum, villosum, bifidum :: laciniis ovatis, acutiusculis, subæqualibus, erectis.

Corolla universalis uniformis, sanguinea.

Propria duplex.

Exterior infundibuliformis, calyce longior : tubus cylindricus, subangulatus, deorsum angustatus, subcoriaceus, persistens ; limbus 5-fidus (sæpe 4-fidus) : laciniis lanceolatis, obtusis, erectis, inæqualibus, unica duplo latiore, deciduis.

Interior pentapetala : petalis ovato-lanceolatis, obtusis, latiusculis, erectis, corolla exteriori fere duplo longioribus : unguibus subulatis, margini tubi corollæ exterioris insertis.

Stam.

Stam. Filamenta constanter undecim, fili-
formia, longissima, h. e. corolla duplo
longiora, erecta, subcurvata, æqualia,
inferne coalita in tubum integrum, an-
tice hiantem, germen cingentem, mar-
gini tubi corollæ exterioris adnatum,
dein in filamenta, basi æqualia, fissum.

Antheræ ovatæ, incumbentes.

Pistill. Germen superum, è tubo corollæ
exterioris pedunculatum : pedunculo
tubo adnato ; cylindricum, tomento-
sum.

Stylus filiformis, longitudine staminum,
inflexus.

Stigma simplex.

Peric. Legumen oblongum, compressum,
circa dissepimentum angustatum, ple-
rumque biloculare.

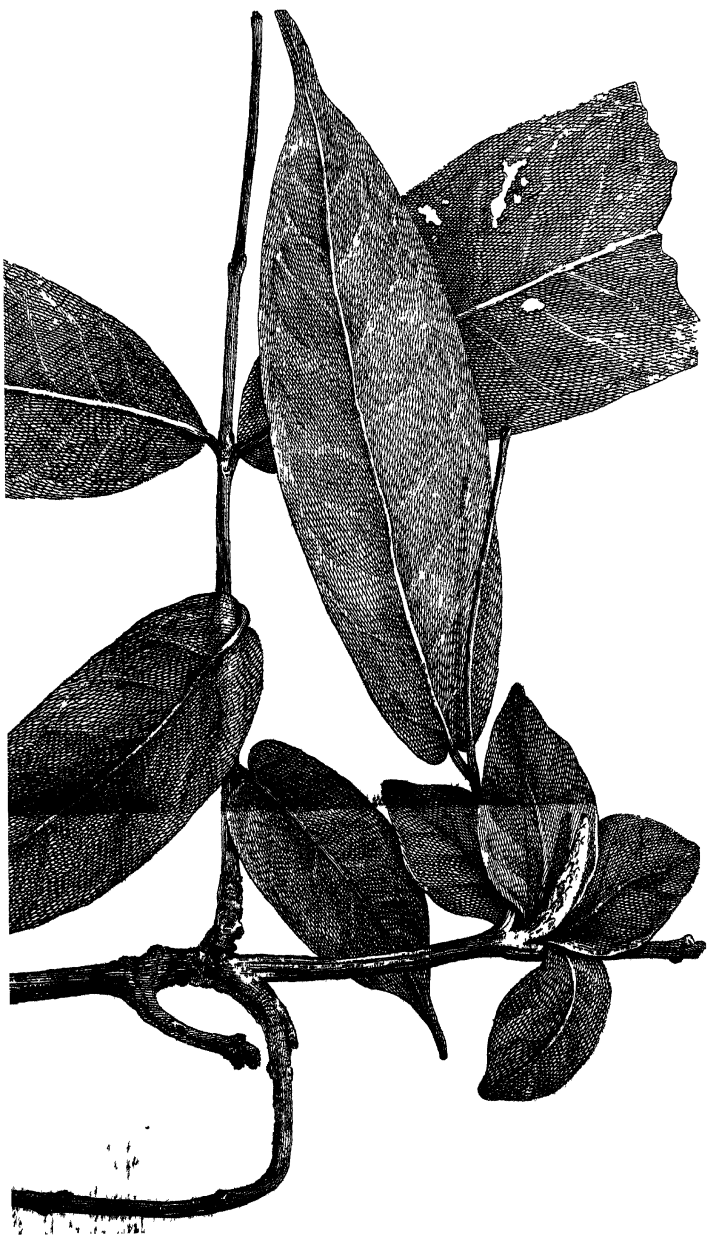
Dissepimento membranaceo.

Semina solitaria, ovata, compressa, rugo-
siuscula, fibris fungosis obvoluta.



Yponomeuta (Haworth & Hillebrand.)

Ph. s. Tra. Vol. I. Tab. 1796



Received Nov. 12, 1772.

XXIII. *Extract of a Letter from the Rev. Samuel Kirkshaw, D. D. Vicar of Leeds, and Rector of Ripley, in the County of York, to William Mountaine, Esq; F. R. S. Communicated by William Watfon, M. D. and F. R. S.*

Read Feb. 11, 1773. **O**N the 29th of Sept. last, about two o'clock in the morning, were three remarkably loud claps of thunder, attended with proportionable lightning. Mr. Thomas Heartly, formerly Wine-merchant, of Leeds, but lately retired from business to Harrowgate, lived there in a hired house, the second northward from the Queen's-head. While he was in bed with his wife, she was awaked from sleep by the thunder, and went to the window; but not being afraid, she got to bed again, and fell asleep. About five she awaked; and, not perceiving her husband to breathe, though warm, endeavoured to awake him—in vain! She quickly sent for Mr. Hutchinson, a considerable apothecary at Knarsborough, who, upon sight of Mr. Heartly, and some experiments, declared him dead, though still very warm. At her request,

request, however, he opened a vein; and Mr. Heartly bled freely, infomuch that the blood did not cease to ooze out of the orifice till the body was put in the coffin, which was on Thursday evening, the first instant, viz. October, and it was not even then cold. His hair, which he wore, was considerably burnt, or singed on the right side of his head, which was uppermost (for he lay then on his left side) and the inside of his night-cap, on the same side, was singed or browned, though no where on the outside marked at all. Within the cap was found a splinter from the bed-post next to his head, which post was torn and split into many splinters or shivers, from the top to the bottom, though a strong oaken post, and almost new. No wound, or mark of any sort, was discovered on any part of his body; but the lower part of his right cheek was swelled, and much hardened. So far I was told by Mrs. Heartly and her servants, but I saw the bed-post.

In the chamber where this happened, there was a small chimney to the north, made up, but not quite close, by a chimney-board, upon which I could not discover any mark or hole, or other indication of the lightning's passing that way.

Between that chimney and the west end of the room, stands the bed, in the north-west corner of the room, close to the west and north walls; the deceased lay next to the west wall, with his head near to the head bed-post, in the north-west corner abovesaid.

There is only one window in the room, full cast, consisting of three pretty large lights, separated

rated by two stone mullions, each light supported by six strong iron bars across it, parallel to the floor, and the intermediate one, rather more than half of it, made into a casement, the frame of which is of iron, and the surrounding frame of the same. In the southermost light, which had three squares of glass in breadth, two of the lowest squares were perforated in or near the middle, about an inch square; but as some small parts of the glass were gone, I could only guess at the size of the holes, nor could distinctly estimate the shape of them, nor form the slightest conjecture, whether the lightning had made its ingress or egress through both, or either of them. The intermediate square of glass left perfectly sound—there was no other iron about the window, except the abovementioned—but the curtain-rods of the bed, which stood about ten feet from the window, I observed, were iron, stronger (larger) somewhat than usual.

Mrs. Heartly lay on Mr. Heartly's left hand, when the thunder was, and felt not the least stroke from the lightning, or perceived any effects from it, except that her right arm, she found, when she awoke, was stunned and benumbed, and a little painful, which continued for a few days, but is now quite well.

I took notice of a pump, which stood about ten or eleven feet from the house, in nearly a right line from the window abovementioned, whose handle (or sraepe, as it is called hereabout) is all of iron, very thick and long, and a strong iron ball for a head to it.

Ripley, Oct. 12, 1772.

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XXIV. 4

XXIV. *Extract of a Letter from Paul Panton Esq; of Plaswgyn in Anglesey, to the Honourable Daines Barrington, V. P. concerning the Increase of Population in Anglesey.*

Plaswgyn in Anglesey, Oct. 3, 1772.

DEAR SIR,

Read Feb. 25, 1773. **I** Wished to have sent you a fuller account of the state of population in this island; but so little care has been taken to preserve the parish registers, that scarcely any that are antient are to be met with. Upon the whole of those I send you, there is great reason to make a pleasing conclusion, that we become more healthy, and increase in population. Heretofore the inhabitants of this island lived chiefly upon fish, with which, especially herrings, these coasts were abundantly furnished. Salted herrings were their principal food. This rambling fish, the herring, having left us, our islanders have neglected pursuing other branches of the fishery, and have betaken themselves more to agriculture. The potatoe plant has not been cultivated in any great quantities here until of late years; but, since the failure of our herring fishery, it has made great part of the food of the inha-

inhabitants. Perhaps the want of the one, and the increased consumption of the other, may be amongst the causes that have contributed to the better health of our people. The increase in population in Llanduvnan and Pentraeth parishes, has not been owing to mines, or any new advantage introduced. The inhabitants are wholly employed in husbandry.

I am,

Dear SIR,

Your very obliged humble servant,

Paul Panton.

BIRTHS and BURIALS, in LLANSADURN Parish,
Anglesey.

Christened.		Buried.		Christened.		Buried.	
1590—	6	—	2	1620—	5	—	5
91—	1	—	4	21—	3	—	4
92—	—	—	2	22—	7	—	7
93—	5	—	5	23—	1	—	6
94—	5	—	4	24—	6	—	1
95—	7	—	3	25—	3	—	2
96—	5	—	3	26—	4	—	6
97—	5	—	7	27—	5	—	2
34—		30		34—		33	

BIRTHS, &c. in LLANSADURN Parish.

	Christened.	Buried		Christened.	Buried.
1750—	8	— 6	1764—	9	— 10
51—	5	— 4	65—	10	— 1
52—	6	— 3	66—	9	— 7
53—	10	— 12	67—	6	— 10
54—	9	— 4	68—	11	— 8
55—	9	— 8	69—	9	— 11
56—	9	— 8	1770—	10	— 16
57—	9	— 3	71—	5	— 5

63—50

BIRTHS, &c. in PENTRAETH Parish.

	Christened.	Buried		Christened.	Buried.
64—	11	— 14	1672—	11	— 10
1665—	7	— 10	73—	8	— 8
66—	6	— 6	74—	10	— 11
67—	6	— 14	75—	13	— 11
68—	8	— 5	76—	10	— 25
69—	8	— 37	77—	19	— 8
70—	7	— 48	78—	19	— 12
1671—	9	— 43	79—	12	— 21

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	Weddings.	Christenings.	Burials		Weddings.	Christenings.	Burials
1740—	2	— 11	— 8	1764—	5	— 20	— 10
41—	2	— 15	— 17	65—	3	— 22	— 7
42—	7	— 6	— 9	66—	1	— 14	— 9
43—	4	— 14	— 18	67—	5	— 19	— 15
44—	8	— 14	— 10	68—	5	— 18	— 11
45—	2	— 12	— 7	69—	2	— 18	— 15
46—	2	— 14	— 5	1770—	8	— 14	— 11
47—	5	— 14	— 11	71—	4	— 24	— 12

BIRTHS.

BIRTHS, &c. in LLANVAIR yn GORNWY Parish.

Christened.			Buried.			Christened.			Buried.		
1732—	10	—	13	1764—	12	—	18				
33—	7	—	7	65—	8	—	11				
34—	12	—	6	66—	16	—	5				
35—	9	—	5	67—	10	—	15				
36—	6	—	17	68—	10	—	8				
37—	10	—	7	69—	19	—	5				
38—	5	—	5	1770—	14	—	9				
39—	9	—	7	71—	12	—	6				

68—67

101.

BIRTHS, &c. in BEAUMARIS Parish.

Christened.			Buried.			Christened.			Buried.			Christened.			Buried.		
1676—	19	—	19	1710—	18	—	26	1764—	44	—	22						
77—	15	—	17	11—	26	—	29	65—	44	—	28						
78—	18	—	19	12—	26	—	21	66—	41	—	38						
79—	18	—	16	13—	22	—	17	67—	32	—	35						
1680—	14	—	24	14—	45	—	24	68—	44	—	28						
81—	16	—	23	15—	31	—	36	69—	37	—	47						
82—	14	—	25	16—	33	—	21	70—	47	—	22						
83—	18	—	31	17—	35	—	38	71—	39	—	29						
135—			174			236—			212			328—			249		

BIRTHS,

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BIRTHS, &c. in LLANDDYFNAN Parish.

Weddings. Christenings. Burials.					Weddings. Christenings. Burials.						
1547—	—	—	5	—	4	1620—	3	—	2	—	2
48—	1	—	4	—	3	21—	2	—	4	—	1
49—	—	—	8	—	3	22—	1	—	1	—	7
1550—	1	—	7	—	6	23—	3	—	6	—	21
51—	1	—	—	—	6	24—	1	—	4	—	18
52—	—	—	3	—	2	25—	4	—	7	—	8
53—	4	—	3	—	5	26—	4	—	12	—	5
54—	1	—	6	—	10	27—	2	—	8	—	5
8—36—39					20—44—67						

Weddings. Christenings. Burials.					Weddings. Christenings. Burials.				
1750—	1	—	7	— 5	1764—	7	—	18	— 14
51—	1	—	9	— 4	65—	5	—	19	— 10
52—	9	—	15	— 6	66—	3	—	26	— 14
53—	6	—	23	— 11	67—	2	—	17	— 17
54—	4	—	9	— 11	68—	2	—	20	— 13
55—	2	—	18	— 4	69—	7	—	24	— 24
56—	2	—	14	— 2	1770—	4	—	16	— 12
57—	3	—	16	— 3	71—	2	—	14	— 4
28—111—46					32—154—198				

XXV. *A Letter to the Rev. Nevil Maskelyne, F. R. S. Astronomer Royal, from Mr. Bailly, of the Royal Academy of Sciences at Paris: Containing a Proposal of some new Methods of improving the Theory of Jupiter's Satellites. Translated from the French, with the Original underneath.*

S I R,

Read Feb. 18 and
25, 1773.

THOUGH I have not the honour of being personally known to you, I flatter myself, you will excuse the liberty I have taken, of communicating to you two methods, of my invention, for perfecting the theory of the satellites of Jupiter. The former of these methods serves to measure their diameters, and the latter is intended to make the observations comparable with each other, though made in different places, and with different instruments. As my intention has been

Monfieur,

Quoique je n'aye pas l'honneur d'être connu de vous, je pense, que vous ne trouverez pas mauvais, que je prenne la liberté de vous communiquer deux méthodes, que j'ai imaginées, pour perfectionner la théorie des satellites de Jupiter. La première de ces méthodes sert à mesurer leur diamètre, et la seconde a pour objet de rendre les observations comparables, soit quelles aient été faites en différens lieux et avec différens instrumens.

Monsieur

been to make these methods useful, I cannot do better, than submit them to the best judges. Your reputation, Sir, engages me, to lay them before you, and to beg you would examine them: flattered as I should be with your approbation, I shall receive, with great gratitude, whatever criticisms they may happen to suggest. Observation must determine, whether, by this method, it will be possible to arrive at an agreement, which hitherto has been wished for, without being attained.

You know, that the observations of the eclipses of the third and fourth satellites, made by different observers, vary from each other 3, 4, and 5 minutes, and sometimes more; and that there is even a pretty sensible difference in those of the second. In the 38th page of the preface to my *Essay on the Theory of the Satellites*, which has been presented to the Royal Society, I mentioned the inequality discovered by Mr. de Fouchy, and I suggested, that the perfect-

ing

Mon bât a été que ces méthodes soient utiles; je ne puis mieux faire, que de les soumettre aux meilleurs juges. Votre réputation, Monsieur, m'engage à vous les détailler, et à vous prier de les examiner: votre suffrage me flatterait infiniment, mais je recevrai toujours avec reconnaissance les réflexions critiques que ces méthodes pourront vous suggérer. Les observations vous feront connoître si par cette méthode on peut parvenir à un accord que l'on a souhaité jusques ici sans l'avoir obtenu.

Vous savez, que les éclipses du troisième et du quatrième satellite, faites par differens observateurs, different entr'elles de 3, 4, 5 minutes et quelquefois plus; et que celles du second different même assez sensiblement. Dans *L'Essai sur la Théorie des Satellites*, dont j'ai fait hommage à la Société royale, je parlais dans la préface, page xxxviii, de l'inégalité découverte par Mr. de Fouchy, et j'an-

nonçais,

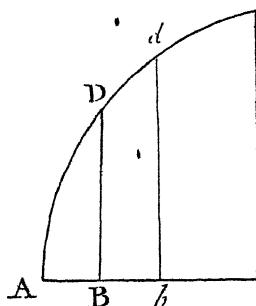
ing of this theory might perhaps depend upon the quantity of this inequality, which Mr. de Fouchy has not determined, not having been at leisure, to resume the subject, since the year 1732. The segment of the disc, which is not eclipsed, when the satellite disappears, must vary in the proportion of the squares of the distances of Jupiter from the Sun, and from the Earth. This is what a little reflection will make evident to every one, and this is the first cause of the inequality. Since Mr. de Fouchy's observation, it has been discovered, that the light of the satellite likewise decreases, in proportion to the proximity of Jupiter's disc; the brightness of the planet weakens that of the satellite, and, for this reason, the eclipses, which happen too near the opposition [of Jupiter to the Sun], are looked upon as defective. Besides, the light of Jupiter, as well as that of his satellites, is different, in his
different

nonçais, que la perfection de cette théorie dépendrait peut être de la quantité de cette inégalité, que Mr. de Fouchy n'a point déterminée; il n'a pu s'en occuper depuis 1732. Il résulte de la considération du segment du disque, qui n'est pas éclipsé, quand le satellite disparaît, que ce segment varie en raison [*inverse*] des quarrés de la distance de Jupiter au Soleil, et de la distance de Jupiter à la Terre. Voilà la première cause d'inégalité. Depuis Mr. de Fouchy, on s'est apperçu que la lumière du satellite diminueoit encore en raison de la proximité du disque de Jupiter; l'éclat de la planète efface celui du satellite, c'est pour cette raison que les éclipses, qui arrivent trop près de l'opposition, sont regardées comme défectueuses. De plus, la hauteur de Jupiter sur l'horison contribué aussi à affoiblir sa lumière, et celle de ses sa-

different elevations above the horizon : when the planet is low, more rays of light are lost, in their passage through a thicker atmosphere ; and whenever the light is less, the segment, which is not eclipsed when the satellite disappears, and which I call the insensible segment, increases, and occasions another inequality in the moment of the eclipses ; lastly, the power of the telescopes, or their aperture, which, according as it is greater or less, gives more or less light, contributes to the variation of this segment. Here then are four causes of inequality, which I reduce to one principle, and the following is the scope of my researches. When the satellite disappears, there is certainly a segment of its disc which remains uneclipsed ; the magnitude of this varies, on account of the four causes just mentioned ; thence it follows, that

tellites : quand cette planete est peu élevée, il se perd plus de rayons en traversant une atmosphere plus épaisse ; or, toutes les fois que la lumiere diminue, le segment, qui n'est pas éclipsé quand le satellite disparait, ce segment que j'appelle le segment insensible, augmente, et il en résulte une autre inégalité dans le moment des éclipses ; la force des lunettes, ou leur ouverture qui, lors qu'elle est plus ou moins grande, fournit plus ou moins de lumiere, contribue encore à faire varier ce segment. Voilà donc quatre causes d'inégalités, que je ramene à un même principe, et quant à mes recherches, voici ce dont il est question. Quand le satellite disparait, il y a certainement un segment de son disque qui n'est pas éclipsé. La grandeur de ce segment varie en raison des quatre causes que nous venons d'établir. D'où il résulte, que

that if in one eclipse the segment is ABD , and in another Abd , when the satellite disappears in the second eclipse, it will have got less into the shade, by a part of its diameter Bb ; which part Bb , therefore, must be the value of the equation between the two eclipses.



Now, if we call Ab , a ; AB , b , the radius of the disc of the satellite r , the semidiameter of the shadow taken from the tables R , and the total duration of the eclipse d , the time taken up in going over Bb , or the equation (^e), will be $\frac{2 R r (a-b)}{d}$, which contains three unknown quantities, to wit, the versed sines a and b , of the two invisible segments, and the semidiameter of the disc of the satellite :

que si pour une éclipse ce segment est ABD , et que pour une autre éclipse il soit Abd , dans cette seconde éclipse, quand le satellite disparaîtra, il sera moins entré dans l'ombre, d'une partie Bb de son diamètre, laquelle partie Bb est la valeur de l'équation entre ces deux éclipses. On trouve facilement que si l'on nomme Ab , a ; AB , b , le rayon du disque du satellite r , le demi diamètre de l'ombre tiré des tables R , et la durée entière de l'éclipse d ; le tems employé à parcourir Bb , ou l'équation, sera $\frac{2 R r (a-b)}{d}$. Cette expression contient trois inconnues, les sinus versés a et b des deux segments invisibles, et le demi diamètre r du disque du satellite. Car vous savez,

tellite: for you know, Sir, that there is nothing to be depended on, in all that has been done upon the diameters of the satellites by Cassini, Whiston, and Maraldi. The following is the way which I have taken, to determine these unknown quantities. I observe, first of all, that two of them, a and b , are reducible to one; because, as you will see presently, the two segments are always in a known proportion [to the whole disc of the satellite, as well as to each other]; and consequently, the proportion of their versed sines, Ab , AB , may be obtained, either by calculation, or by a table made for the purpose. In order to discover it, considering that when the satellite disappears, it is from the diminution of its light, I conceived, that one might contrive to imitate, at any time, what happens in the eclipses, by diminishing the light. I have an achromatic telescope of five feet length, and 24 lines aperture. I made some diaphragms of pasteboard, which I could apply on the

Monfieur, tout ce qui a été fait sur les diametres des satellites par Cassini, Whiston, et Mr. Maraldi, et vous savez qu'il n'y a rien sur quoi on puisse compter. Voici comment je suis parvenu à les déterminer. Je remarque d'abord que les deux inconnues, a , b , se réduisent à une seule, parceque, comme vous le verrez tout à l'heure, les deux segments sont toujours dans un rapport connu, et conséquemment par le calcul ou par une table dressée exprès, on a le rapport de leurs sinus versés Ab , AB . Pour les connaître, j'ai vu que quand le satellite disparaît, c'est par la diminution de la lumière, et j'ai pensé, qu'à tous les momens on pouvait imiter, d'une manière factice, ce qui arrive dans les éclipses, en diminuant la lumière du satellite. J'ai une lunette achromatique de 5 pieds, qui porte 24 lignes d'ouverture. J'ai taillé des diaphragmes de carton, que je pouvais appliquer

the outside of my object-glass, the openings of which lessened, by half-lines successively, from twenty-four lines down to three. In fine weather, I applied these successively to my object-glass, and endeavoured to find out, whether, by trying from the greatest to the less, some one of them could not be found, that would make the satellite disappear. My success in this gave me great satisfaction. One day, for instance, the third satellite disappeared, when the opening was reduced to three lines, and the first, when it was reduced to six only; and as, in the telescopes, the quantity of light is in the proportion of the squares of the apertures, I concluded, that the 64th part of the light of the third satellite, and the 16th of the first, were insensible; whence it follows, that if, at the instant of an eclipse of the first satellite, the sixteenth part of its light is insensible, the invisible segment ABD will be likewise a sixteenth part of the disc; and thence it will be easy to compute

appliquer extérieurement sur mon objectif, et dont les ouvertures diminuaient de demie ligne, depuis 24 lignes jusqu'à 3. Par un beau tems, j'ai placé successivement sur mon objectif tous ces diaphragmes, et j'ai cherché, si en passant toujours des plus grands aux plus petits, il n'y en aurait pas quelqu'un, qui fit disparaître le satellite; et j'ai réussi avec une grande satisfaction. Un certain jour, par exemple, le troisième satellite disparut, lorsque l'ouverture fut réduite à trois lignes, et le premier, lorsque l'ouverture fut réduite à six seulement. Comme dans les lunettes la lumière est dans le rapport du carré des ouvertures, j'en ai conclu que la 64^{me} partie de la lumière du troisième, et la seizième du premier étaient insensibles: d'où il résulte, que si dans le moment d'une éclipse du premier satellite la seizième partie de sa lumière est insensible, le segment invisible ABD sera aussi la seizième partie du disque, d'où il est aisé de calculer le sinus

verie

compute the versed sine A B. In these first observations, I took care to chuse the time, when the satellite was at its greatest elongation ; for the insensible part increases prodigiously, and sometimes amounts to a third of the disc, when the satellite is very near the edge of Jupiter. This variation is much larger, than that which takes place, in consequence of the distance of Jupiter from the opposition to the Sun, and contrary to it. As it is scarce possible to estimate the law of the variations of this segment, occasioned by the proximity of the disc of Jupiter, I judged, that they ought to be determined by observation. Accordingly, I followed the satellite from the edge of the disc of Jupiter, to the farthest limit of its eclipses, that is, with respect to the first, to the distance of two semi-diameters of Jupiter. Having thus several points by observation, I got the rest by interpolation, and made a table of the variations of the invisible segment,

verse A B. J'avais eu soin dans ces premieres observations, de choisir le tems où le satellite était dans ses plus grandes digressions. Car cette partie insensible augmente prodigieusement, et devient quelquefois le tiers du disque, lorsque le satellite est fort près du bord de Jupiter. Cette variation est contraire et beaucoup plus grande que celle qui a lieu en vertu de la distance à l'opposition. Comme on ne peut gueres estimer la loi que suivent les variations de ce segment, à raison de sa proximité du disque de Jupiter, j'ai jugé qu'il falloit déterminer ces variations par observation. J'ai donc suivi le satellite depuis le bord du disque de Jupiter, jusqu'au terme le plus éloigné de ses éclipses, c'est à dire, à l'égard du premier, jusqu'environ à la distance de deux demi diametres de Jupiter. Ayant obtenu ainsi plusieurs points donnés par observation, j'ai eu les autres par interpolation, et j'ai dressé une table des variations du segment invisible, qui dépendent de la distance au bord de

ment, which depend upon the distance from the edge of Jupiter; a similar table I likewise, made for each of the three first satellites; but have not yet been able, to make sufficient observations on the fourth. These tables are contained in a long paper of mine, which will be published in the volume of our Academy for 1771; but, if you please, I will send them to you. These segments being known, it is clear, that, besides their variations occasioned by the distance of the satellite from the edge of Jupiter, they will be liable to others. First, In consequence of the change of Jupiter's distances, both from the Sun and from the Earth. Upon this account, the magnitudes of these segments being known, for a particular epoch*, those known magnitudes must be multiplied by $\frac{m^2 n^2}{p^2 q^2}$, to determine the magnitude of the segment at any other time. In which expression, p and q stand for the di-

* *Known*, by the author's tables, for any distance of the satellite from the edge of Jupiter, at the particular epoch to which the tables are adapted.

stances

de Jupiter; j'ai dressé une pareille table pour chacun des trois premiers satellites; je n'ai pu observer encore, comme il faut, le quatrième. Si vous desirez ces tables, Monsieur, je vous les enverrai. Elles sont dans un long mémoire, qui sera imprimé parmi ceux de l'Académie pour 1771. Ces segments étant une fois connus, il est clair, qu'outre les variations que ces segments subissent, en vertu de la distance du satellite au bord de Jupiter, ils varieront encore. 1°. En vertu de la distance de Jupiter au Soleil, et de Jupiter à la Terre. Il faudra donc multiplier ces segments, connus pour une certaine époque, par $\frac{m m n n}{p p q q}$, en supposant que p et q aient été les di-

stances

stances of Jupiter from the Sun and from the Earth respectively, at the given epoch, and m and n for the distances, at the other time, for which the value of the invisible segment is required. 2dly, There will be other variations depending on Jupiter's height above the horizon. The segments which I have observed, have all been reduced to the constant height of 15° . Mr. Bouguer, in his Optics, has given a table of the degrees of light of the planets, at their different elevations above the horizon, which, from my own observations, I have found to be very exact, and useful for the present purpose. Now, as the segments are in the inverse ratio of the numbers of this table, putting g for the number corresponding to the elevation of 15° , and b for the number corresponding to any other elevation, the segments must be multiplied by $\frac{g}{b}$. 3dly, These segments will yet be subject to another variation depending on the aperture of the telescope. It is certain that a larger

stances de Jupiter au Soleil et à la Terre, au moment de cette époque, et m , n , les distances de Jupiter au Soleil et à la Terre, au moment pour lequel on voudra connaître le segment invisible. 2. En vertu de la hauteur de Jupiter sur l'horizon. Les segments, que j'ai observés, ont été tous réduits à la hauteur constante de 15° . Mr. Bouguer a donné, dans son Optique, une table des degrés de la lumière des planètes, à différentes hauteurs sur l'horizon ; je me suis assuré, par des observations, qu'elle est très exacte, et très utile dans la recherche présente. Les segments sont en raison inverse des nombres de cette table : soit donc pour 15° ce nombre g , et pour une hauteur quelconque soit ce nombre b , il faudra multiplier les segments par $\frac{g}{b}$. 3. En vertu de l'ouverture de la lunette. Il est certain qu'une ouverture plus grande

larger aperture giving more light, the insensible part of the disk must be smaller ; and it seems demonstrable by theory, that this insensible part, or the invisible segments, must be inversely as the squares of the apertures. I resolved, however, to assure myself of this by experiment. For this purpose, I carried my telescope to Mr. Messier's observatory, who has one of Dollond's telescopes, of three feet and a half length, and forty lines aperture. On the 20th of August 1771, he saw the second satellite disappear in his telescope, through an aperture of three lines. The same satellite disappeared in mine, when the aperture was reduced to the same quantity of three lines, and not before. We changed instruments, and, repeating the experiment, found the same effect. Now, the insensible part was $\frac{9}{1600}$ of the disc, in Mr. Messier's instrument, and $\frac{9}{376}$ in mine. These portions, therefore, in these telescopes, were in the inverse proportion of the squares of the apertures.

donnant plus de lumiere, la partie insensible du disque doit être plus petite, et il paraît démontré, par la theorie, que, dans différentes lunettes, cette partie insensible, ou les segmens invisibles doivent être en raison inverse des quarrés des ouvertures ; mais j'ai voulu m'en assurer par l'expérience. Je me suis transporté chez Mr. Messier avec ma lunette ; il en a une de Mr. Dollond, de trois pieds et demi, qui porte 40 lignes d'ouverture. Le 20 Août 1771, il a vû disparaître, dans sa lunette, le second satellite, par une ouverture de 3 lignes. Le même satellite n'a disparu, dans la mienne, que par une ouverture également de 3 lignes. Nous avons repété la même expérience, en troquant d'instrumens, et nous avons trouvé les mêmes résultats. Or, dans la lunette de Mr. Messier, la portion insensible était $\frac{9}{1600}$ ^{me} du disque, et dans la mienne $\frac{9}{376}$. Ces portions, dans ces lunettes, étaient donc en raison inverse des quarrés des ouvertures.

apertures. Conſéquemment, in order to determine the ſegments for an aperture of any number of lines k , the ſegments of my table, which are all calculated for an aperture of 24 lines, muſt be multiplied by $\frac{576}{k^2}$. Hence, to compute the inviſible ſegment for any particular eclipse, the actual diſtance of the ſatellite from the edge of Jupiter being known, look for the quantity of the inviſible ſegment, which anſwers to that diſtance, in my table, and multiply this quantity by $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. If two different obſervations made in the ſame place, or rather two obſervations, made in different places by different obſervers, are to be compared, the inviſible ſegment muſt be determined, ſuch as it was for each obſerver; AB and Ab , the verſed ſines of theſe ſegments,

ainſi, mes ſegmens ayant é é déterminés pour une ouverture de 24 lignes, ſi on veut les avoir, pour une lunette, dont l'ouverture aura un nombre de lignes exprimé par k , il faudra multiplier ces ſegmens par $\frac{576}{k^2}$. Donc, quand on voudra calculer le ſegment inviſible, qui aura lieu pour le moment d'une certaine éclipse, au moien de la diſtance actuelle du ſatellite au bord de Jupiter, on cherchera, dans mes tables, le ſegment inviſible, qui y répond, et on le multipliera par $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. Si on veut comparer enſemble deux obſervations différentes, ou plutôt, ſi on veut comparer deux obſervations faites en différens lieux par deux obſervateurs différens, on déterminera le ſegment inviſible, qui a lieu pour chacun de ces obſervateurs: on calculera les ſinus verſés Ab et AB de ces deux ſegmens, et dans l'expression

2 R r ($a - b$)

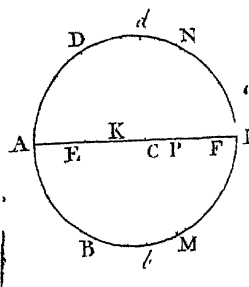
segments, must be computed, and in the expression $\frac{2Rr(a-b)}{d}$, the only remaining unknown quantity, will be r . The following is the method I have hit upon for determining it. I considered, that, by trying different diaphragms successively, some few minutes before an immersion, it would be easy to find out the particular size, which would make the satellite disappear; and that the proportion of the invisible segment to the whole disc of the satellite, for that instant, would, by that means, be determined. Suppose then that I have found this diaphragm: my next step is, to cover the object-glass of my telescope with a diaphragm somewhat larger, which suffers me just to perceive the satellite, but so weak and small, that the least further diminution of its light must render it invisible. I wait till it actually disappears; I write down this time, then take away the diaphragm, and the number of seconds, which pass betwixt this first disappearance and the

$\frac{2Rr(a-b)}{d}$, on n'aura plus d'autre inconnue que r . Voici

la méthode que j'ai imaginée pour la déterminer. J'ai pensé, que si quelques minutes avant une immersion, on essayait différents diaphragmes, jusqu'à ce qu'on ait trouvé celui, qui fait disparaître le satellite, on aurait, pour ce moment, le rapport du segment invisible au disque entier du satellite. Ce diaphragme étant connu, je couvre ensuite l'objectif de ma lunette d'un diaphragme un peu plus grand, qui me laisse appercevoir le satellite, mais faible et très petit, de manière que ce satellite cessera d'être visible dès que sa lumière sera tant soit peu diminuée. J'attends qu'il disparaisse, [et je suis ainsi averti du moment où il commence à toucher l'ombre] (b) je marque cet instant, j'enlève le diaphragme, et le nombre de secondes écoulé, entre cette première disparition

the true immersion, giving me a great part of the diameter, I easily compute the whole. The following is an example of my method. On the 26th of June 1771, there was an immersion of the third satellite, at 56' after nine in the evening. I found the diaphragm, which made the satellite disappear, to be of 12 lines. Then I fitted my glass with a diaphragm of 17 lines; I might have taken one much smaller: presently the satellite disappears. But removing the diaphragm, I see the satellite again, very distinctly, for 2' 18"; after which, the true immersion followed. Now, this is my calculation. The aperture of the diaphragm, which made the satellite disappear, being 12 lines by observation, the invisible segment at the instant of the eclipse, must have been

a quarter of the disc. Let ABD be this quarter. I know that, at the instant of the immersion, the satellite had entered the shade, by the whole part EF of its diameter. I say then, if on an aperture of 24 lines, the part ABD is insensible, the insensible part, on an aper-



et la véritable immersion, me donne la mesure d'une grande partie du diamètre, d'où il est aisé de conclure le diamètre entier. Voici un exemple de la méthode. Le 26 Juin, 1771, à 9 h. 56' du soir, il y avait une immersion du troisième satellite. J'ai déterminé de 12 lignes le diaphragme qui faisait disparaître le satellite. J'ai garni ma lunette d'un diaphragme de 17 lignes (je l'aurais pu prendre beaucoup moins ouvert) j'ai vu disparaître le satellite, j'ai ôté le diaphragme, j'ai revu, très distinctement, le satellite pendant 2' 18", après quoi s'est faite la véritable immersion. Voici le calcul. J'apprends, par l'observation du dia-

phragme

aperture of 17 lines, will be larger than A B D, in the proportion of the square of 24 to the square of 17. Saying, then as $17^2 : 24^2 :: 0, 25000 :: x$, x comes out $= 0,49827$, or near half the disc represented by unity; thence I see that, at the instant of the first disappearance, the satellite had not gone in farther than K. Putting the radius $AC=1$, the versed sines A E, A K will be $= 0,59602$ and $0,99884$; consequently, $EK = 0,40282$. Substitute this value of E K, instead of $a - b$, in the expression $\frac{2R \times r (a-b)}{d}$, and you will have $\frac{2Rr \times 0,40282}{d} = 2' 18''$, and $r = 5' 23''$ of time, will be the semidiameter of the satellite, or, the whole diameter will be $22' 34''$ [of the satellite's orbit, considered as a circle, or, would be seen, under an angle of this quantity, from Jupiter's centre]. In this observation, as I have already

phragme de 12 lignes, qu'au moment de l'eclipse le segment invisible a dû être le quart du disque : soit A B D le quart du disque; je suis sûr, qu'au moment de l'immersion, le satellite était entré dans l'ombre, de toute la partie E F de son diamètre. Je dis ensuite, si pour une ouverture de 24 lignes la partie A B D est insensible, cette partie doit devenir plus grande pour une ouverture de 17 lignes; et elle doit augmenter dans le rapport du carré de 17 au carré de 24: faisant donc comme $17^2 : 24^2 :: 0,25000 : x$ on trouve $x = 0,49827$, ou près de la moitié du disque, représenté par l'unité. Je vois donc, qu'au moment de la première disparition le satellite n'était entré encore que jusqu'en K. En supposant le rayon $AC = 1$, on trouve les sinus versés A E, A K égaux à $0,59602$, et $0,99884$: par conséquent, $EK = 0,40282$. Si l'on substitue, pour $a - b$, cette valeur de E K, dans la formule $\frac{2Rr (a-b)}{d}$, on aura $\frac{2Rr \times 0,40282}{d} = 2' 18''$: d'où l'on tire $r = 5' 23''$ de tems, pour le demi diamètre du satellite, ou $22' 34''$ de degrés, pour le diamètre entier. Dans cette observation, comme je l'ai déjà remarqué,

ready said, I used a diaphragm with too great an opening (c), for the first disappearance. Take then a second observation. On the first of August 1771, there was an emerfion of the third fatellite, about 15' after nine. I marked the instant of this emerfion; then I furnished my telescope with a diaphragm of 8 lines. The fatellite disappeared, and did not begin to appear again till at the end of 6' 24". Some minutes after, when it was quite come out of the shadow, I measured the diaphragm, which would make it disappear, and found it of 7 lines. These 7 lines give a segment ABD of 0,08507. Then saying $8^2 : 24^2 :: 0,08507 : x$; x , or the segment ANM, comes out = 0,76562, AE = 0,27994, AP = 1,43098, and EP = 1,15104. Therefore $\frac{2Rr \times 1,15104}{d} = 6' 24''$. From this equation r comes out, 5' 20'', and the whole diameter 22' 22'', [in parts of the circular orbit]. These two conclusions agree

fo.

marqué, j'avais employé un diaphragme d'une trop grande ouverture, pour la premiere disparition. Voici une seconde observation: Le 1 Août 1771, à 9 h. 15', il y avait une émerfion du troisieme. Je marquai l'instant de cette émerfion, ensuite je garnis ma lunette d'un diaphragme de 8 lignes: le fatellite disparut, et il ne commença à reparaitre qu'au bout de 6' 24"; quelques minutes après, étant tout à fait sorti de l'ombre, je mesurai le diaphragme qui le faisoit disparaître, il se trouva de 7 lignes. 7 lignes donnent un segment ABD de 0,08507. Faisant ensuite $8^2 : 24^2 :: 0,08507 : x$, on trouve x ou le segment ANM = 0,76562: on trouve AE = 0,27994, AP = 1,43098, et EP = 1,15104. On a donc la formule $\frac{2Rr \times 1,15104}{d}$

= 6' 24'' : d'où l'on tire $r = 5' 20''$, et le diametre entier, en degrés de 22' 22''. Ces deux resultats s'accordent merveilleusement bien,

fo perfectly well with each other, that, if I am not too fond of my own work, I may venture to fay, the method, I have invented, may be carried to great exactnefs. I hope you will have the goodnefs to give me your opinion of it, which I have the greateft refpect for, and will be very ufeful to me, efpecially if you have leifure to repeat the obfervations. I fhall take the liberty to fubjoin a few hints, upon the manner of making them, at the end of this letter. The diameter of the firft fatellite I have determined by three obfervations as follows;

An	Immersion	$\left\{ \begin{array}{l} 1771, \\ \text{June } 30, \end{array} \right\}$	$\left\{ \begin{array}{l} 7' \\ 7' \\ 7' \end{array} \right\}$	$\left\{ \begin{array}{l} 17'' \\ 17'' \\ 3'' \end{array} \right\}$	or	$\left\{ \begin{array}{l} 1' \\ 1' \\ 1' \end{array} \right\}$	$\left\{ \begin{array}{l} 45'' \\ 46'' \\ 29'' \end{array} \right\}$	as
	Emerfion	$\left\{ \begin{array}{l} \text{Aug. } 1, \end{array} \right\}$	$\left\{ \begin{array}{l} 7' \\ 7' \\ 7' \end{array} \right\}$	$\left\{ \begin{array}{l} 3'' \\ 3'' \\ 1'' \end{array} \right\}$	time of	$\left\{ \begin{array}{l} 1' \\ 1' \\ 1' \end{array} \right\}$	$\left\{ \begin{array}{l} 45'' \\ 46'' \\ 29'' \end{array} \right\}$	feen
	Emerfion	$\left\{ \begin{array}{l} \text{Sept. } 2, \end{array} \right\}$	$\left\{ \begin{array}{l} 7' \\ 7' \\ 7' \end{array} \right\}$	$\left\{ \begin{array}{l} 1'' \\ 1'' \\ 1'' \end{array} \right\}$	in	$\left\{ \begin{array}{l} 1' \\ 1' \\ 1' \end{array} \right\}$	$\left\{ \begin{array}{l} 45'' \\ 46'' \\ 29'' \end{array} \right\}$	from
								24.

You fee, Sir, that this agreement is likewise very fatisfactory. Mr. Meffier took part in thefe obfervations, and found the application of them very eafy. He him-
felf

bien, et fi je ne m'abufe pas trop fur mon travail, il me femble, que la méthode, que je viens de vous expofer, eft fufceptible d'une très grande exactitude. J'efpere que vous voudrez bien m'en dire votre fentiment, dont je fais le plus grand cas, et qui me fera très utile, furtout fi vous avez le loifir de répéter ces obfervations. Je prendrai la liberté d'ajouter, à la fin de cette lettre, quelques réflexions fur la maniere de les faire. A l'égard du diamètre du premier, je l'ai déterminé par trois obfervations. L'immersion du 30 Juin 1771, m'a donné fon diamètre de 7' 17'' en tems, ou de 1° 1' 45'' vû de Jupiter. Le premier Août l'émerfion m'a donné fon diamètre de 7' 3'' en tems, ou de 0° 59' 46''; enfin, l'émerfion du deux Septembre m'a donné fon diamètre de 7' 1'' en tems, ou de 0° 59' 29''. Vous voyez, Monsieur, que cet accord eft encore très fatisfaisant. Mr. Meffier a bien voulu prendre part à ces obfervations, il en a trouvé l'ufage facile.

self observed the diameter of the second satellite, by the emerfion of the 30th of Auguft; but it emerged at fo small a diftance from the firft, that this circumftance may have vitiated the obfervation; the diameter of this fmallite muft therefore be verified by frefh obfervations. However, the refult of Mr. Meffier's makes it $7' 2''$ in time, or $29' 42''$ feen from Jupiter; and a former obfervation of mine, of the 11th of July, gave the fame quantity precifely. Thus, by means of the tables given in my paper, which I had the honour of mentioning to you, it will be poffible, to compute the invifible fegment, for all the obfervations which have been hitherto made, and, the diameter of the fmallite being likewise afcertained, to reduce the instant of the obferved eclipse to that of the paffage of the center over the edge of the fhadow, which will be a fixed term for all the obfervations, and

Il a obfervé lui même le diamètre du fecond, par l'émerfion du 30 Août, mais il fortit fi près du premier, que cette circonftance peut avoir nui à la bonté de l'obfervation; ainfi le diamètre de ce fmallite a befoin d'être confirmé par de nouvelles obfervations. Quoiqu'il en foit, il réfulte de fon obfervation, que le diamètre du fecond eft de $7' 2''$ en tems, ou de $29' 42''$ vû de Jupiter. Je l'avois trouvé par une autre obfervation du 11 Juillet, de $7' 2''$, précifément comme Mr. Meffier. Au moïen des tables, que j'ai données dans mon mémoire, et dont j'ai eu l'honneur de vous parler, Monsieur, on pourra donc calculer, pour toutes les obfervations faites jufqu'à préfent, le fegment invifible, qui a eu lieu, et, au moïen du diamètre connu réduire l'inftant de l'éclipse, au moment où le centre s'eft trouvé fur le bord de l'ombre, moment qui fera un terme fixe pour toutes les obfervations, et pour tous les obfervateurs,

and all the observers, who but seldom agree in the observation of the same eclipse. I confess, that the transparency of the air is not always the same, and that a greater or less degree of transparency will make the segments smaller or larger, and consequently affect the observation. The inequality of sight may likewise occasion some error; for, though it might be possible, to settle the general effect of the difference of sight of different observers, the sight of the same person is not constantly the same, and even independently of the change produced by age, may not have the same strength at all times. But by the method I propose, all these inconveniencies will be remedied, in future observations, with little trouble. Every observer is to furnish himself with several diaphragms of pasteboard, gradually diminishing by half-lines, to be applied to the object-glass externally, and some minutes before an immersion, or after an emersion, he is to determine which of them
intercepts

teurs, qui ne s'accordent que rarement dans l'observation de la même éclipse. Je conviens, que la transparence de l'air n'est pas toujours la même, et que quelques degrés de plus ou de moins dans cette transparence rendent les segments moins ou plus grands, et conséquemment affectent l'observation. L'inégalité des vuës est encore une autre source d'erreurs; quand même on pourrait établir l'effet de la différence des vuës des observateurs; ces vuës ne sont pas constantes, et, sans parler du changement, que l'âge amène avec lui, la vuë peut n'avoir pas la même force dans tous les instans. Voici la méthode, que je propose, pour remédier à tous ces inconveniens, d'une manière facile et commode, à l'égard des observations futures. Il suffirait, que chaque observateur fit des diaphragmes de carton, pour appliquer extérieurement à son objectif, qui diminuassent de demi ligne en demi ligne. Quelques minutes avant une immersion, ou après une émer-
sion,

intercepts from him the sight of the satellite. Having found this, and knowing likewise the diameter of the satellite, he will reduce [by the process of calculation already explained] the observed instant of the eclipse, to that of the passage of the center ; which is the same [as I said before] for all the observers in the world. You see, Sir, what advantages would arise, from this agreement, for the theory of the satellites, and the precision of the terrestrial longitudes. This method takes in every thing ; the difference of glasses, that of sights, the greater or less transparency of the atmosphere, &c. Observation gives the segment greater or less, in proportion to the combined influence of all these causes. The principal advantage of this method, which requires only a very simple calculation, is, that it depends on no hypothesis. It enables us to measure immediately the light of the satellite, whether increased or diminished by all the causes above-mentioned ; to measure, I say, the real impression of that

il déterminerait, lequel de ces diaphragmes lui dérobe la vue du satellite : ensuite, au moyen du diamètre, il réduirait l'instant de l'éclipse observée, au moment du passage du centre, moment qui est le même pour tous les observateurs de tous les pays du monde. Vous voyez, Monsieur, quels avantages il résulterait de cet accord, pour la théorie des satellites, et pour la précision des longitudes terrestres. Cette méthode renferme tout ; la différence des lunettes, celle des vues, le plus ou moins de transparence de l'atmosphère, &c. C'est en raison de toutes ces causes, que l'on trouve, par observation, le segment plus ou moins grand. L'avantage principal de cette méthode, qui n'exige qu'un calcul fort simple, est, de n'être soumise à aucune hypothèse. On mesure immédiatement la lumière du satellite, augmentée ou diminuée par

that light upon the eye, whatever be the actual state of the organ. These, Sir, are the methods and enquiries which I submit to your judgment, and with that view, I have been very copious in my details. I am highly ambitious of your approbation, and shall pay great attention to your remarks. I must add, that I am sensible, the determination of the invisible segment, by means of the diaphragm, might be inconvenient to those, who make use of large telescopes for the eclipses of the satellites, were it not, that this observation may be equally well made with a smaller telescope, provided only, that it be sufficient to see and distinguish the four satellites; and after the diaphragm is determined by this smaller telescope, the larger one may be used for the observation of the eclipse. For these measures are easily transferred from one instrument to another, the invisible segments in different telescopes, being inversely

as

par toutes les causes que nous avons indiquées, et on mesure même l'impression de la lumière sur l'œil, quel que soit l'état actuel de cet organe. Voilà, Monsieur, les méthodes et les recherches, que je soumets à vos lumières; et je l'ai fait avec beaucoup de détail, afin de vous mettre dans le cas de les juger. J'ambitionne extrêmement votre suffrage, et je ferai usage de vos réflexions. J'ajouterai, que la détermination du segment invisible, par le moyen du diaphragme, pourrait devenir incommode, pour ceux qui se servent de grandes lunettes, dans les éclipses des satellites; mais cette observation peut se faire également avec une petite lunette, pourvu qu'elle fasse bien voir et distinguer les quatre satellites. Quand on aura déterminé le diaphragme avec cette lunette, on fera l'observation de l'éclipse avec son instrument ordinaire. On peut transporter ces mesures, très aisément, d'un instrument à l'autre, puisque les seg-

as the squares of the apertures. For reflectors, I have a method of the same kind with the former, grounded, at least, upon the same principles, by which I can determine their power, and compare them, both with each other, and with the refracting telescopes. I shall conclude with some hints concerning the observation of the diaphragm, for determining the invisible segment. To repeat these observations with judgement, it will be necessary to recollect the intention of them; which is, to measure what portion of the disc remains illumined, that is, what portion of the satellite's light [continues, though unperceived, to be transmitted to the observers eye] at the instant when the satellite disappears, upon the brink of an eclipse. In lessening gradually the aperture of the glass, the observer should not begin with too small an opening; because the eye, not accustomed to

mens invisibles sont, dans les différentes lunettes, en raison inverse du carré des ouvertures. Quant aux télescopes de réflexion, j'ai une méthode à peu près du même genre que celle-ci, et qui du moins est fondée sur le même principe, par laquelle je puis déterminer la force des télescopes, et les comparer, tant entr'eux, qu'avec les lunettes. Je finirai, Monsieur, par quelques réflexions sur l'observation du diaphragme, qui détermine le segment invisible. Pour bien répéter ces observations, il faut se rappeler l'esprit, dans lequel elles ont été entreprises, et dans lequel elles doivent toujours être faites. C'est de mesurer la quantité de lumière, la portion éclairée du disque, qui subsiste encore au moment où le satellite disparaît, en s'éclipsant. Quand on diminue, par degrés, l'ouverture de la lunette, il ne faut pas commencer par une trop petite ouverture: l'œil n'est pas fait à la grande obscurité qui en résulte; on ne verrait point le satellite. A chaque fois

to the great obscurity which follows, might not see the satellite at all. As the opening is gradually contracted, the satellite seems to grow less. The observer sometimes loses sight of it for a moment; but, if he continues to look attentively, he sees it again. The real disappearance is only to be concluded, when, upon fixing with steady eyes, for about half a minute, on the place it occupied, it is seen no more; for if one persisted to observe it much longer, it might happen, that it might be seen to glimmer at times, and immediately disappear. I have always made it a rule, to consider the debilitation of the light, in this degree, as actual disparition, and it is necessary, that observers should agree upon this point, in order that their different estimations may be consistent. These fits, of momentary glimmering and extinction, are undoubtedly owing to the motion of the particles of the atmosphere. In the clearest weather, there are always particles of vapour floating in it, in vast abundance; according as these particles place themselves

fois qu'on diminue l'ouverture, on voit diminuer le satellite: quelquefois on croit ne le plus voir, mais, après y avoir fait attention, on le revoit: il faut le juger disparu, quand on ne l'appergoit plus, après avoir considéré, avec des yeux bien reposés, pendant une demie minute environ, la place qu'il occupe: car, si on s'attachait à le considérer plus longtems, il pourrait arriver, qu'on le revit, par moment, paraître, comme par éclairs, & disparaître aussitôt. Je l'ai toujours jugé disparu, quand il a été réduit à cet état d'affaiblissement; il est essentiel de convenir de ceci, afin que les mesures se rapportent. Ces variations sont dues, sans doute, au mouvement de l'atmosphère. Il y a toujours, dans les

momens

selves in the direction of the ray of light, or out of it, the light of the satellite is diminished or restored, and the satellite, in consequence, is either hid or rendered visible. This does not happen in eclipses, wherein a great part of the light is in reality extinguished. But, in the case I am now speaking of, though the diminution of the aperture of the glass does indeed take away a great quantity of it, yet this quantity is always relative to the actual state of the atmosphere (^d): if that state changes, this quantity becomes alternately sensible or insensible, according as the light meets with more or less obstructions, in its passage, from the vapours. Another thing, which it will be necessary to point out to you, is, that the operation with the diaphragms, for determining the invisible segment, must be made and concluded, before the satellite has touched the shadow. The proper time, therefore

momens (ù le tems est le plus serein, une infinité de vapeurs, dont les particules nagent dans l'atmosphère; selon que ces particules s'accroissent sur la route du rayon de lumière, ou qu'elles s'en écartent, elles ôtent ou rendent plus de lumière à ce satellite, qui, en conséquence, se cache ou se laisse voir. C'est ce qui n'arrive point dans les éclipses, où une grande partie de la lumière est réellement détruite. Ici l'ouverture diminuée de la lunette en détruit réellement aussi une grande quantité, mais cette quantité est toujours relative à l'état actuel de l'atmosphère. Si l'état de l'atmosphère change, cette quantité devient alternativement sensible ou insensible, selon que les vapeurs lui opposent plus ou moins d'obstacles. Une chose qu'il est encore nécessaire de vous faire observer, Monsieur, c'est, que l'opération du diaphragme, qui détermine le segment invisible, doit être faite et finie, avant que le

fore, for beginning this observation, will be determined, by the time the diameter takes in entering, which, in the perpendicular ingrefs, or when Jupiter is in the nodes, is 7' for the two first, and 11' for the third. The time of the oblique ingrefs is $(7') \frac{2R}{d}$ for the two first, and $(11') \frac{2R}{d}$ for the third ; which, for this last, may in extreme cases amount to about 27' or 28'. It is proper to take 5' more ; for the observations of the diaphragm will take up 2', even when use has rendered them familiar, and the tables may be 2' or 3' behind. At present, it is be sufficient, to begin the observation 16' before an eclipse of the third satellite ; but there are times, in which it would be necessary, to begin 29' or 30' before. It is essential, not to begin too late, for fear of missing the observation ; it is likewise essential, not to begin too soon, because then the
segment

le satellite ait touché l'ombre. Ainsi le tems, que le diametre met à entrer, doit donc regler cette observation ; c'est à dire, 7' pour les deux premiers, et 11' pour le troisième ; mais ce tems est celui de l'entrée perpendiculaire, qui n'a lieu que lorsque Jupiter est dans les nœuds. L'entée oblique est $7' \frac{(2R)}{d}$ pour les deux premiers, et $(11') \frac{2R}{d}$ pour le troisième, ce qui peut aller pour celui ci jusqu'à 27' ou 28'. Il faut encore prendre 5' de plus, parceque même étant exercé, il faut bien 2' pour l'observation du diaphragme, et que les tables peuvent être en retard de 2' à 3'. Actuellement il suffit donc, de s'y prendre, pour cette observation 16' avant une eclipse du troisième, mais il y a des tems, où il faudroit s'y prendre 29' ou 30' avant. Il
est

segment measured would be too small, as the satellite is continually either approaching Jupiter, or receding from him. All this is hastily explained; but these matters are so familiar to you, that you cannot but understand me, and this letter is already too long. I am afraid it will tire you; but I am extremely desirous of having the exactness and utility of these two methods, the one, for the measure of the diameters, the other, for making all the observations capable of mutual comparison, ascertained, by repeating the observation of the diaphragm in every eclipse. I cannot take a better way, than to consult the several astronomers, who, like you, besides being deeply skilled in the theory, are the most celebrated observers. If they will adopt this method, it will be the best way of making it general, as others will follow it of course. I have communicated

est essentiel de ne s'y pas prendre trop tard, pour ne pas manquer l'observation; il est essentiel aussi de ne s'y pas, prendre trop tôt, parcequ' alors on mesurerait un segment trop petit; car le satellite s'approche ou s'éloigne continuellement de Jupiter. Tout ceci est expliqué en courant, comme vous voyez, Monsieur, mais vous en savez trop sur ces matieres, pour ne me pas entendre, et cette lettre est déjà d'une excessive longueur. J'ai bien peur, qu'elle ne vous cause de l'ennui; mais j'ai extrêmement à cœur, de constater l'exactitude et l'utilité de ces deux méthodes, celle de la mesure des diametres, et celle de rendre toutes les observations comparables, en répétant, à chaque éclipse, l'observation du diaphragme. Je ne puis mieux faire que de consulter les astronomes, qui étant comme vous, Monsieur, profondément versés dans la theorie, sont, en même tems, les plus célèbres observateurs. Si ceux ci veulent bien adopter cette méthode, c'est le meilleur moyen de la rendre générale: les autres l'adopteront aussi.

municated it to Mr. Messier, who proposes making use of it. Mr. Maraldi, who is gone to his house near Nice, has tried the observation of the diaphragm, with success, with an achromatic telescope three feet and a half long; and he would already have made use of it, but that it is impracticable with the telescope of 15 feet, which he uses for the eclipses of the satellites. I have written to him, that he may observe the eclipse with his usual telescope, and the diaphragm with the achromatic; so that I make no doubt he will use this method, as soon as Jupiter shall have come out of the rays of the Sun. These, Sir, are the things on which I wish to consult you, and have your advice. I shall be
much

Je l'ai communiquée à Mr. Messier, qui se propose d'en faire usage. Mr. Maraldi, qui, comme vous savez, est allé chez lui à Perinaldo, dans le comté de Nice, a essayé l'observation du diaphragme, qui lui a réussi, avec une lunette achromatique de 3 pieds et demi; et il l'auroit déjà employée, si ce n'est, que l'observation du diaphragme n'est pas praticable avec sa lunette de 15 pieds, dont il se sert pour les éclipses des satellites. Je lui ai mandé, comme je vous l'ai fait observer plus haut, qu'il pouvoit faire l'observation du diaphragme avec sa lunette achromatique, et celle de l'éclipse avec sa lunette ordinaire: ainsi je ne doute point, qu'il ne fasse usage de cette méthode, lorsque Jupiter se dégagera des rayons du soleil. Voilà, Monsieur, toutes les choses, sur lesquelles je suis bien aise de vous consulter, et de vous demander votre avis. Si vous croyez que cette lettre en vaille la peine, je serai très flatté, que vous la communiquiez à la Société Royale, à laquelle je présente l'hommage de mon profond respect.

much flattered by your communicating this letter to the Royal Society, if you think it deserves attention.

I have the honour to be, &c.

Bailly,
Of the Royal Academy of Sciences
of Paris.

Pardon, Monsieur, de vous avoir détourné si longtems. Permettez moi de vous assurer du respect avec lequel je suis,

Votre très humble

Et très obeïssant Serviteur,

BAILLY,
De l'Academie des Sciences de Paris.

NOTES ON THE FOREGOING PAPER,

BY THE REV. SAMUEL HORSLEY.

$$(a) \frac{2 R r (a - b)}{d}.$$

This formula is deduced from the following principles.

1st. That the motion of the satellite, in its orbit, is uniform, or, at least, may be considered as such, without sensible error, in the present investigation.

2. That the time, which the semidiameter takes to enter the shadow, in any eclipse, is inversely as the whole time of the duration of the eclipse.

3. That the time, which any given part of the semidiameter takes to enter the shadow, is to the time which the whole semidiameter takes to enter, as that part to the whole.

Now, let a and b denote the versed sines of the arcs $A d$, $A D$ (in the figure p. 189.) respectively, the radius being unity. Let R denote the half-time of the duration of an eclipse, when Jupiter is in the node of the satellite's orbit. r , the time which the semidiameter takes to enter the shadow in such eclipses; d , the whole duration of an eclipse, happening when Jupiter is at any given distance from the node. Then will $\frac{2 R r}{d}$ express the time, which the semidiameter of the satellite will take to enter the shadow, in the eclipse whose duration is d (by 2^d, because $d : 2 R = r : \frac{2 R r}{d}$). And, $\frac{2 R r}{d}$ being the time that the semidiameter takes to enter the shadow, $\frac{2 R r (a - b)}{d}$ will be the time that the part $B b$ takes to enter, by 3^d."

It is to be observed, that, to compare two eclipses by this formula, it is necessary, that the planet should have been at the

same distance from the node of the satellite's orbit, at the commencement of both. For comparing eclipses otherwise circumstanced, a more general formula may easily be deduced from the same principles. If Ad be the insensible segment in one eclipse, AD in another (vide figure p. 189.), a the versed sine of the arc Ad , b of AD (the radius being unity), d the whole duration of the first eclipse, δ of the second, then $\frac{2Rr}{d\delta} \times \overline{\delta a - db}$ is

what the author would call the equation, between the two, arising from the different magnitudes of the insensible segment, or the time, by which the interval between the observed eclipses, differs from the interval between the real passage of the center in each eclipse. This is a general formula, for all eclipses of the same satellite. If the planet's distance from the node hath been the same in both, then $\delta = d$, and this formula changes into the author's. The more general one is here given, rather for the fuller explication of the theory, than for any necessity that there is to have recourse to it in practice. For, though the use of it may sometimes be convenient, eclipses of the same satellite may always be compared without it, when once the diameter of the satellite is known, and the magnitude of the insensible segment in each eclipse determined, by reducing the observed immersion or emersion to the true ingress or egress of the center.

(*b*) The words printed in the Italic character are designedly omitted in the translation, it being apprehended, that it is owing to some inadvertency, that they appear in the author's text. For unless they are expunged, the general description, here intended, of the author's method of determining the diameters of the satellites, will by no means agree with the examples of that method immediately subjoined. These words imply, that the author takes the instant of the disparition of the satellite, in the contracted aperture of the diaphragm, for the moment of the contact of the satellite's limb with the edge of the shadow, and makes that moment, so determined, the basis of his calculations. reasoning, as it should seem, thus. "When any part of the diameter of the satellite, however small, hath entered the shadow, some part of the light, which the observer receives, through the aperture of the diaphragm, from the whole unshaded disc, will be intercepted. But that aperture is so small, that the light transmitted through it, from the whole unshaded disc, is but just sufficient to be sensible; and must, therefore, cease to be so,

when it is in the smallest degree diminished, i. e. when the very smallest part imaginable of the disc is shaded. Therefore, the moment of the disparition, in the aperture of the diaphragm, is the true commencement of the eclipse, or differs from it by less than any assignable difference."

But it appears, from the examples given afterwards, that the author's calculations proceed upon much safer principles. Having determined the portion of the disc, that is insensible on the whole given aperture of his telescope, he computes what larger portion will be insensible, on the smaller given aperture of his diaphragm. And then, by observing the two disparitions, the earlier one in the diaphragm, the other in the telescope with the object-glass uncovered, the last of which he calls the true immersion, he knows the time, in which a given portion of the diameter enters the shadow, and, consequently, the time, in which the whole enters; which determines the magnitude of the whole, in parts of the satellite's orbit, or its apparent magnitude to an observer at Jupiter's center.

(c) The disadvantage of using too great an aperture is, that the part of the diameter obtained by observation, from which the whole is to be concluded, will be less, than the same method of observation would give, with a more contracted aperture. For the larger the aperture of the diaphragm is, which is applied to the object glass, the less is the difference between that aperture and the whole aperture of the telescope; consequently, the less is the difference between the segments, which are insensible in these apertures severally, and the less the portion of the diameter, which passes over the shadow's edge, between the two disparitions.

(d) In eclipses, when once the satellite hath disappeared, or is become visible, the author says, we are not to expect those fits of glimmering and extinction, which he hath described as taking place, when we observe the uneclipsed satellite with very contracted apertures.

The reason is plainly this. In immersions, a part of the disc is still indeed enlightened, when the satellite disappears; and the quantity of light, transmitted from this part to the observer's eye, must be very different, in different states of the air's transparency; and consequently, the satellite, after having disappeared, might become visible again, by a sudden increase of the air's transparency in the tract of the satellite's light, provided,
the

the magnitude of the unshaded part remained, at the instant of the increased transparency, what it was, when the satellite first disappeared. But, as this is not the case, as the unshaded part is continually growing less, the satellite cannot re-appear, unless the increase of transparency be such, as to overbalance the diminution of light made by the progress of the eclipse. And the motion into the shade is so quick, that this can rarely, if ever, happen. By the like reasoning, fits of extinction are not to be expected, when once the satellite hath shewn itself in an emerſion.

The author of these remarks doth not imagine, that any apology is necessary for the liberty he hath taken. He hath the highest opinion of the merit of Mr. Bailly's invention; and this hath excited him, to contribute what he could, to obviate objections, and to prevent mistakes.

XXVI. *A Letter from Mr. Bernard, of Deptford, to Mr. Robertson, Lib. R. S. containing a short Account of an Explosion of Air, in a Coal-Pit, at Middleton, near Leeds in Yorkshire..*

S I R,

Read Feb. 25, 1773. **I** Have at length procured, from my father, a memorandum made by him on the spot, of the effects of foul air set on fire, which I have copied exactly. If it affords you the least satisfaction, I shall be well pleased, though late, in having fulfilled my promise.

I am, with great respect,

Yours,

Grove-Street, Deptford,
January 12, 1773.

W. Barnard..

M E M O-

M E M O R A N D U M.

BEING engaged in Middleton wood, the estate of — Brandling, Esq; near Leeds in Yorkshire, in directing the falling and barking of a large quantity of timber bought of him in May 1758, I was witness of the following accident. Some miners, being to renew their operations on the shaft of a coal-pit, which, in a former year, had been sunk to the depth of sixty yards, in order to get through a stratum of very hard stone, thought proper to drill holes, and fill them with gunpowder. They afterwards, from the top, threw down fire to blast the stone, which made a report little louder than that of a pistol; but the blaze, setting the foul air on fire, produced an effect truly shocking. The whole wood was shaken, the works at the mouth of the pit were all blown to pieces, and the explosion was such as cannot be described. The vacuum in the air was so considerable, that oak trees of a load or more each, at a great distance from the pit's mouth, that before stood upright, stooped towards the pit very much, and must have fallen wholly down, had not the air been instantly replaced. The bark-pullers, at a quarter of a mile from the pit, were so alarmed by the shaking and explosion, that not one of them would have remained in the wood, had they attempted to blast it again.

N. B. The trees in the whole circuit stooped towards the pit.

PHILOSOPHICAL
TRANSACTIONS.

VOL. LXIII. PART II.

PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT

OF THE
Present Undertakings, Studies, *and* Labours,

OF THE
INGENIOUS,

IN MANY
Considerable Parts of the WORLD.

VOL. LXIII. PART II.

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XXVII. *Extract of a Register of the
Barometer, Thermometer, and Rain, at
Lyndon in Rutland, 1772, by T. Barker,
Esq; Communicated by Sir John Pringle,
Bart. P. R. S.*

Read March 4, 1773.

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the house.			Abroad.			
					High.	Low.	Mean.	High	Low.	Mean	
Jan.	Morn.	29.94	28.35	29.26	44	31	36	45	10	30	2.145
	Aftern.				45	30½	37	51	25	35½	
Feb.	Morn.	29.65	28.65	29.11	46	29	37	46	13	31	3.477
	Aftern.				45½	30½	38	52	24	38½	
Mar.	Morn.	29.64	28.61	29.20	47½	33	41	47	23	34	2.346
	Aftern.				49	34½	42	57	32	43½	
April	Morn.	29.90	28½	29.50	51	37½	44½	50	27	39	0.882
	Aftern.				52	40	46	57	35	48	
May	Morn.	30.02	29.07	29.66	55	45½	50½	55	33½	45	1.869
	Aftern.				56	46	52	68	47	55½	
June	Morn.	29.99	29.07	29.66	68	51½	59½	66	47	56	3.890
	Aftern.				70	53½	61	81	58½	68	
July	Morn.	29.99	28.96	29.61	65	59½	62	64	48½	56	0.891
	Aftern.				66	60½	63	73	62	68	
Aug	Morn.	29.91	28.95	29.51	67	56½	61½	61	44	54½	1.678
	Aftern.				68	60	63	76	61	67½	
Sept	Morn.	29.84	28.57	29.42	62½	52½	58	61½	40	51	4.515
	Aftern.				65	54	59	71	54½	60½	
Oct.	Morn.	29.95	28.77	29.52	58	51½	55	57	39	48	3.267
	Aftern.				59	52	56	64½	47½	57½	
Nov	Morn.	29.93	28.50	29.26	55½	41	47	56	31½	41	2.461
	Aftern.				56	42	48	59	39½	46½	
Dec.	Morn.	30.06	28.78	29.57	48½	36	42½	50	24	38	1.226
	Aftern.				49½	36½	43	51½	28½	40½	

January began mild, but soon inclined to frost; and about the middle of the month, a severe season set in; much frost, and great snows, which would have been very great indeed, if it had been all lying together; but they were intermixed with thaw, rain, and floods, and continued to the middle of March. This caused a great expence of hay, and, with the very backward spring, and frequent cold weather, even to the middle of May, made few people have any considerable quantity of hay left. There were, however, some intervals of mild weather, and more grass this spring than last.

The summer was, in all the south of England, very dry, and burning. There was so little grass about London, that many were forced to fodder their cattle, even in the height of it; but in all the middle of England, the summer was a very fine one; no cold weather, nor in general very hot, but chiefly very fair and fine, and a sufficient quantity of rain came, whenever we wanted it: so great a quantity of hay, so well got, was hardly ever known. The beginning of harvest was also well got in, but the latter part of it, for it was a late one, was, in this country and northward, exceeding bad; and in this wet weather, some wheat suffered, most of the barley, and all the beans and pease. The crop of wheat, where it was well gotten, was tolerable good, the barley yielded worse, white pease were plentiful enough, but ill got, beans and grey pease universally a failing crop, much of the seed being burst in the wet seed-time. This bad latter part of harvest was in September, which was all very wet, as it was also, in a less degree, till the beginning of December,

ember, but so warm, that the grafs grew till the middle of October as fast as in summer; but the wheat seed-time was bad, which did but indifferently on wet land, though it came up very well on light soils. The season continued mild and open, the grafs springing, and scarce a morning that could be called frosty, till December 22, when a calm and moderate frost concluded the year.

Wet summers make plenty of grafs, but drier seasons are more favourable to corn. The most plentiful ten years in my time were from 1741 to 1750, which were also the driest; and the most failing series of crops have been since that remarkable wet year 1763; since which, there has been, in general, much more rain than before.

P. S. Be pleased to correct the following errata, in my former letter, Phil. Transf. Vol. LXI. p. 223.

	For	Read
Feb. 10.	E. by W.	E. by N.
16.	E. by W.	E. by N.
Aug. mean 36 to 70.	2.194.	2.184.
Nov. 1737.	9.570.	9.570.

XXVIII. *Observations on the Lagopus, or Ptarmigan; in a Letter from the Hon. Daines Barrington, V. P. R. S. to Mathew Marry, M. D. F. R. S.*

DEAR SIR,

THE many different specimens of Lagopus, both in their winter and summer plumage, which have lately been presented to the Royal Society, from Hudson's Bay, enable us to correct many mistakes that have hitherto been made in the description of this bird; as well as the unnecessarily multiplying the species of the Tetrao genus.

As that able and ingenious naturalist, M. de Buffon, is the last ornithologist who hath made any observations on this bird, it may not be improper to take notice of some of his supposed inaccuracies.

The Lagopus, of which M. de Buffon gives an engraving, is in its winter plumage; and the feet of the bird are consequently covered very thick with feathers. M. de Buffon, however, from not having examined the specimens of the Lagopus with proper attention, says, that Aristotle could not have been acquainted with this bird, because the under parts of the claws are entirely covered with feathers; which circumstance is so very striking and peculiar, that it could not have escaped this father of natural history.

If a winter specimen, however, of the Lagopus, or Ptarmigan, is accurately examined, it will be found,

found, that no feathers grow precisely under the claws; though, by wrapping very thickly round them, they have very strongly that appearance: and, in a summer specimen, not only the feet, but even the legs, are rather bare of plumage. If Aristotle, therefore, had procured the bird in its summer dress, he could not have observed this very striking circumstance, which M. de Buffon relies upon as so strongly characteristic.

The same difference between the plumage in summer and winter is experienced in each of the three species of Tetrao, which have (according to one of Linnæus's subdivisions) feathered feet; and it is usually said with us, that they have in winter their snow-boots. M. de Buffon, therefore, unjustly charges the author of the British Zoology for supposing, that this is a wise provision of Nature against the inclemency of the season, when he says, [a] that the vrogallus Minor, or our Black Cock, hath not the same protection for its feet, though it buries itself under the snow, and, becoming torpid, equally wants such additional warmth.

With regard to the torpidity of this bird, M. de Buffon relies upon Linnæus's asserting, that *sape sepelitur in nive* [b]; which by no means signifies that the bird is torpid, but only that it buries itself, sometimes, under the snow; as sheep do with us in the more rigorous seasons, when it lies very deep in the mountains.

[a] T. ii. p. 216.

[b] Linn. S. Nat. p. 159. This circumstance is also observed by Pontoppidan, Pt. ii, p. 75. Engl. Transl.

The Black Cock, however, is so far from being torpid in the winter, that it even approaches the habitation of man when distressed for food; and I shall likewise conclude, till I see a specimen which proves the contrary, that, like the other Tetraos, whose feet are covered low with feathers, this part of the plumage becomes thicker in winter.

M. de Buffon also seems to be mistaken in supposing, that the thick plumage round the feet is peculiar to the Lagopus; as it is believed, that Linnaeus's first division of this genus have all of them the same additional cloathing for the winter; nor is this extraordinary warmth confined merely to this genus, as the noble specimen of the large White Owl, which hath lately been presented to the Royal Society from Hudson's Bay, is covered about the claws with a plumage of perhaps an equal thickness.

The next remarkable circumstance in this bird is, that the shafts of many of the wing-feathers are black; which M. de Buffon supposes to be only six; whereas they are eight in the specimens from Hudson's Bay; the two last are, indeed, of a fainter colour.

M. de Buffon next says [c], that Brisson counts eighteen feathers in the tail; and Willoughby, sixteen; which he reduces himself to fourteen. It seems to me, however, that Willoughby's number is the more accurate; and, by examining the difference between the summer and winter specimens, I find that the black feathers of the tail are covered by two upper ones, which in summer are brown, and in winter white.

I cannot also discover, in any of the specimens, the two white feathers in the tail, according to Linnæus's description, *restricibus nigris apice albis, intermediis albis*, as the two covering feathers before-mentioned cannot, with propriety, be termed *intermedii*; nor are they white in the summer, but brown: so that Linnæus makes a circumstance, which varies with the season, to be a permanent characteristic of the bird.

M. de Buffon next supposes [d], that Willoughby and Frisch speak of different birds under the name of *Lagopus*; because the first says, that the feet are covered with soft, and the latter, with harsh and bristly feathers. The remarks, however, of these ornithologists, are easily reconciled; for, if the finger is drawn according to the course of the feathers, they feel soft; and, if in the contrary direction, harsh and bristly. The difference also between Belon, Gesner, and Linnæus, with regard to the call of this bird, is as easily accounted for; because most male birds differ from the female in this respect, and sometimes the young birds from those which are full-grown.

This naturally brings me to shew, that M. de Buffon (who hath great merit in other parts of his *Natural History*, by not unnecessarily multiplying the species of animals,) hath, in this kind of *Tetrao*, considered as two species what, when properly examined, will turn out to be only the *Lagopus*, or *Ptarmigan*.

His chief reason for considering the *Lagopus* of Hudson's Bay, as being distinct from the *Ptarmi-*

[d] T. ii. p. 274.

gan, arises from his asserting, that Mr. Edwards, in his description of that bird, says, that it is twice as large. Mr. Edwards, however, only considers the size of the Hudson's Bay Lagopus as between that of a Pheasant and a Partridge; in which he is very accurate: the bird is not only evidently so to the eye, but weighs three ounces more than a common Partridge [c].

M. de Buffon likewise seems to make an unnecessary species of Tetrao, under the name of *le petit Tetras, à plumage variable*; as his principal argument for this opinion is, that they are not found on the mountains, as the Lagopi are.

Now, it is very clear, from the name given in the catalogue from Hudson's Bay to this bird, of the *Willow Partridge*, that it lives entirely in that part of the world on the plains; nor are there (it is believed) any very high mountains in the neighbourhood of our forts.

When M. de Buffon, therefore, conceives, that the Lagopus is always endeavouring to find out snow and ice, and that it carefully avoids the glare of the sun [f]; it should seem, that the observation is by no means generally true; because, though the rigour of a Hudson's Bay winter is great, yet the summer is very pleasant, and the snow soon disappears, without which M. de Buffon imagines that the bird cannot exist; though his ninth plate represents the Ptarmigan, in his winter dress, sur-

[a] The Partridge, when full-grown, weighs thirteen ounces, and the Ptarmigan, sixteen.

[f] T. ii. p. 272.

rounded with trees and plants in most luxuriant foliage and vegetation.

I cannot agree, moreover, with M. de Buffon, when he says, that the flesh of the Lagopus is bitter; as I have eat them myself in the Highlands of Scotland; nor should I have been able to distinguish the taste from that of the common Grouse, which is well known to be a bird of most excellent flavour.

What I have hitherto remarked is chiefly in answer to that ingenious naturalist, M. de Buffon, who is the last ornithologist that hath both described and engraved this bird. I have only one new observation to make myself; which is, that the claws are scooped off at the end exactly like a writing-pen (wanting indeed the slit); which circumstance may likewise be seen in the claws of our common Grouse, or Heath-game, though the resemblance is not quite so strong as in the Ptarmigan.

I shall now conclude with copying, from the catalogue transmitted with the specimens from Hudson's Bay, what further relates to the Lagopus; which, as I observed before, is there called a Willow-partridge [g].

“ The Willow-partridges gather together in large
 “ flocks in the beginning of October, harbouring
 “ amongst the willows, the tops of which are their
 “ principal food; they then change to their winter

[g] It is not at all extraordinary, that it should there be considered as a Partridge; because the White Partridge is the name given to this bird by the old ornithologists, who have very naturally considered edible birds nearly of the same size, as Partridges when they have short tails, and as Pheasants, when they have long ones.

“ dress. They change again in March, and have
 “ their complete summer dress by the latter end of
 “ June. They make their nest in the ground in
 “ dry ridges; and are so plentiful, that ten thou-
 “ sand have been killed in the three forts in one
 “ winter.”

I am, dear S I R,

Your most faithful,

humble Servant,

Daines Barrington.

XXIX. *Account of the Effects of Lightning at Steeple Ashton and Holt, in the County of Wilts, on the 20th of June, 1772, contained in several Letters, communicated by Edward King, Esq; F. R. S.*

TO EDWARD KING, Esq;

SIR,

Read March 18, 1773. **I** Have sent you, agreeably to your request, some account of a storm of thunder and lightning that happened at Steeple Ashton, in Wiltshire, on the 20th of June last. I was from home when it happened; but the truth of it is attested by the Reverend Mr. Wainhouse of Steeple Ashton, and the Reverend Mr. Pitcairn of Trowbridge, who were in my house during the tempest, and were in danger of losing their lives by it. I have added a description of the effects of the same storm upon a house at Holt, in this county. Both tend to prove in a remarkable manner the danger of placing any considerable quantity of iron in the upper part of chimnies, without a conductor to guard against the strokes of lightning. You are at liberty to lay these papers before

before the Royal Society, if you think them deserving of their attention.

I am,

SIR,

Your obedient

humble Servant,

Steeple Ashton,
Aug. 10, 1772.

L. ELIOT, Vicar of Steeple
Ashton, in Wiltshire.

ON the 20th of June, 1772, between twelve and one o'clock in the afternoon, a violent storm of thunder and lightning happened at Steeple Ashton, in Wiltshire. During the storm, a woman in the village saw a large quantity of lightning come out of a cloud, part of which is supposed to have fallen on the top of the north chimney of the vicarage house, attracted probably by an iron hoop that went round the chimney, and by some iron bars placed within it, that formerly made part of an apparatus to prevent its smoking. That the lightning fell on these iron bars is very probable, because the colour of two of them that were contiguous was changed, nine or ten inches in length, to a dark blue, like that of a watch spring, no uncommon effect of electrical fire.

In the north parlour, to which this chimney belonged, were the Reverend Mr. Wainhouse, of Steeple Ashton, and the Reverend Mr. Pitcairn, of Trowbridge, the former standing, and the latter

ter sitting in a great chair, with his back to the fire-place, near the wire of a bell. In the south parlour, separated from the other by a hall, were a maid servant and a painter; in the kitchen another maid servant; in the coal-house, four or five yards from the house, a man servant; near the barn, about fifty or sixty yards from the house, another man servant. When the lightning fell upon the house, the man servant near the barn heard a very loud noise, equal, he supposes, to the sound of twenty cannons fired at once, and would have fallen to the ground, if he had not caught hold of something to support himself. The other man servant in the coal house was struck backward, and felt something, as he describes it, like a stream of warm water poured upon the middle of his body, which, if it was not the electric fluid itself, was the heated air expanding itself with violence after the explosion. The maid in the kitchen heard a great noise, but received no shock. The other maid servant, who was standing near *the middle* of the south parlour, suffered likewise no shock, being only terrified exceedingly with the explosion, and the sparks of fire, which she saw on all sides of her; but the painter, who was in the same room, painting near the chimney and the bell wire, was struck on the left side of his body that was next the wire, from his head to his waist; he felt in particular a severe shock, like the electrical one, in his left wrist, which was marked all round with blue and yellow intermixed; a splinter from the wooden case, that covered the bell-wire, struck through his glove, and wounded his hand; and he was stunned for some time.

it may be proper to observe, that immediately after the woman had seen the lightning come from the cloud, as above-mentioned, some persons in the village, besides those in or near the vicarage house, were thrown to the ground.

The following is the account, which Mr. Wainhouse and Mr. Pitcairn give of what happened in the north parlour in which they were. As they were conversing about a loud clap of thunder that had just happened, they saw on a sudden a ball of fire between them, upon a level with the face of the former, and about a foot from it. They describe it to have been of the size of a sixpenny loaf, and surrounded with a dark smoke; that it burst with an exceeding loud noise, like the firing of many cannons at once; that the room was instantly filled with the thickest smoke; and that they perceived a most disagreeable smell, resembling that of sulphur, vitriol, and other minerals in fusion; insomuch that Mr. Pitcairn thought himself in danger of suffocation. Mr. Wainhouse providentially received no hurt, except a slight scratch in his face from the broken glass that was flying about the room, a kind of stupefaction for some time, and a continued noise in his ears, which noise, the effect of the explosion, happened likewise to Mr. Pitcairn, and others in the house.

The lightning fell on Mr. Pitcairn's right shoulder, made a hole in his coat, about a quarter of an inch in diameter, went under his arm in one line to his breast, descended from thence down the lower parts of his body in two irregular lines, about half an inch broad, attracted probably by his

his watch, the glass of which it shivered into small pieces, and meeting perhaps with a little resistance from it, spread itself round his body, and produced the sensation of a cord, tied close about his waist. A violent pain in his loins immediately followed; and from thence to his extremities there seemed to be a total stoppage of circulation, all sensation being lost, and his legs and feet resembling in colour and appearance those of a person actually dead. Besides shivering the glass of his watch, the lightning melted a little of the silver of it, and a small part also of half a crown in his pocket. When it came to the middle of his thigh, it left an impression of a blackish colour, resembling the branch of a tree, which in a few days disappeared; but the lines on his body are still visible, and are of a dark blue, intermixed irregularly with a deep yellow. From the middle of his thigh the lightning changed its direction again, and went down the under side of it to the calf of his leg, and so to his shoe, which *was split into several pieces* in so remarkable a manner, as justly to claim the inspection of the curious. As soon as Mr. Pitcairn was struck, he sunk in his chair, but was not stunned; his face was blackened, and the features of it distorted. His body was burned in several places, small holes were made in different parts of his cloaths, and he lost in some measure the use of his legs for two or three days; but by proper care he soon recovered, except a weakness and numbness in his right leg, which still remains. What is remarkable, Mr. Pitcairn remembers very well to have seen the ball of fire in the room for

a short time, a second or two, *after he found himself struck with the lightning.* Extraordinary as this circumstance may appear, it may be proper to take notice, that it is entirely agreeable to an observation of the learned and ingenious Dr. Franklin, quoted below *.

The effects of the lightning on the building and furniture were as follows. The north chimney was thrown down, the roof and ceiling near it beat in; large stones were forced out of the walls, some were driven to a considerable distance, one in particular to about 200 feet. The glass of the windows in the north parlour and the chamber over it was forced outwards, except in the casements, which were open, and in which not a pane of glass was broken. The case of a clock in the same parlour fell forwards, and was beaten to pieces; a looking glass over the chimney was thrown on the floor, and broken, some of the quicksilver was melted, as was likewise some of the lead belonging to the windows. A bureau, that was locked, was opened; as was also the parlour door, inwards, probably by the external air rushing in to restore the equilibrium. Some bedding in one of the chambers was fired, but the fire was extinguished of itself, or by the rain that fell during the storm, before it was discovered. Several splinters were torn out of a hoghead full of

* In every stroke of lightning I am of opinion that the stream of the electric fluid, &c. will go considerably out of a direct course for the sake of the assistance of good conductors; and that in this course it is actually moving, though silently and imperceptibly, *before the explosion*, in and among the conductors, &c.

Franklin's Experiments and Observations on Electricity, edit. 4. pag. 124.

beer, but the cask was not materially damaged, nor the beer spilt. The iron bell-wire in both the parlours and the hall was reduced to smoke and entirely dissipated, excepting in those parts where it was twisted, and double, and also the wire springs contiguous to the bell, which the lightning left undamaged, as well as the brass handles and bell itself. The cieling and wall on each side, where the wire went, was stained irregularly, a foot or more in breadth, with a dark blue intermixed with a deep yellow. It is worth observing, that this iron bell wire was very small, considerably less than a common knitting needle ; but though it was itself destroyed, yet it seems to have served as a conductor to the lightning, and to have prevented worse effects than happened. For when the lightning had run along, and consumed all the *single* wire, and had reached that which was twisted and double in the south parlour, contiguous to the brass handle, which the bell used to be rung with, it made a hole in the wall of five or six inches in diameter, being attracted probably by an iron stove on the other side in the kitchen chimney, where meeting with several large conductors, andirons, poker, tongs, &c. it seems to have been conveyed into the ground. This appears probable, because the progress of it below stairs could not be traced beyond this hole, which it made in the wall. In the chamber over the kitchen, a small piece of wood was indeed struck out of a bed post, and the glass of half a window was driven outwards ; but this does not seem to have been the immediate effect of the lightning, but of the shake from the explosion.

Upon the whole, it must be allowed, that Mr. Wainhouse and Mr. Pitcairn had a most wonderful and providential escape, particularly the last gentleman; for had the lightning passed from his shoulder in a right line through his body, instead of going round upon the surface of it, under his arm, in order to come at his watch, by which it seems to have been attracted, immediate death would in all probability have been the consequence.

Whether Steeple Ashton is from its situation particularly exposed to thunder-storms, is uncertain. It may however be proper to mention, that in the year 1670, July the 25th, a violent storm of thunder and lightning damaged the church steeple, which was 93 feet high; and on the 15th of October in the same year, another thunder storm threw it entirely down, and killed two of the workmen, who were repairing it.

We have perused, and carefully examined the above account, and hereby testify the truth of all the facts related in it, to the best of our knowledge and belief.

August 22,
1772.

William Wainhouse.
Robert Pitcairn..

To Edward King, Esquire.

P. S.

I fear the description I left at your house of the thunder storm last June is too long; you may shorten it as much as you think proper. I purposely omitted a few circumstances, which I thought less material. Since I was in London, I have been informed of two particulars, which I will beg leave to mention to you.

Mr. Field, a painter of Trowbridge, during the storm, observed a ball of fire vibrate forward and backward in the air over some part of Steeple Ashton, and at last dart down perpendicularly, which in all probability was the ball of fire that Mr. Wainhouse and Mr. Pitcairn saw in the north parlour of the vicarage house.

The other circumstance is as follows: After the explosion of the ball of fire in the north parlour, Mr. Pitcairn observed a great quantity of fire of *different* colours *vibrating* in the room forwards and backwards with a most extraordinary swift motion.

The vibration in both these cases observed by different persons before and after the explosion is a fact, which I should have taken notice of, had I been made acquainted with it sooner.

I am, dear Sir,

Your obliged and affectionate

humble servant,

Steeple Ashton,
near Trowbridge, Wilts,
Nov. 28, 1772.

L. Eliot.

To

To the Rev. Mr. ELIOT.

SIR,

Hearing of the damage done by lightning to your house, on the 20th of June last, I have sent you an account of what happened to mine on the same day, and nearly at the same time.

During the storm a person in this place saw a body of fire moving towards a house that is next to mine, though at some distance from it; attracted probably by a large iron bar of ten or twelve feet long, fixed horizontally to support a high chimney. This body of fire changed its direction, and fell on my house, forced a brick out of the chimney, near that part of it to which the iron bar was fastened, and went through the house to an outward door on the opposite side, which happened to be open; there it burst with a loud noise, like the firing of cannons, and filled the room where I was with smoke and the smell of sulphur. I was fortunately three or four feet out of the line in which it moved. I was however struck against the wall near which I stood; my body was covered with fire, and I thought for some time I should have been suffocated with smoke and the smell of sulphur; but by the blessing of Providence I escaped unhurt, and my house received no damage.

I respectfully am,

SIR,

Your most humble Servant,

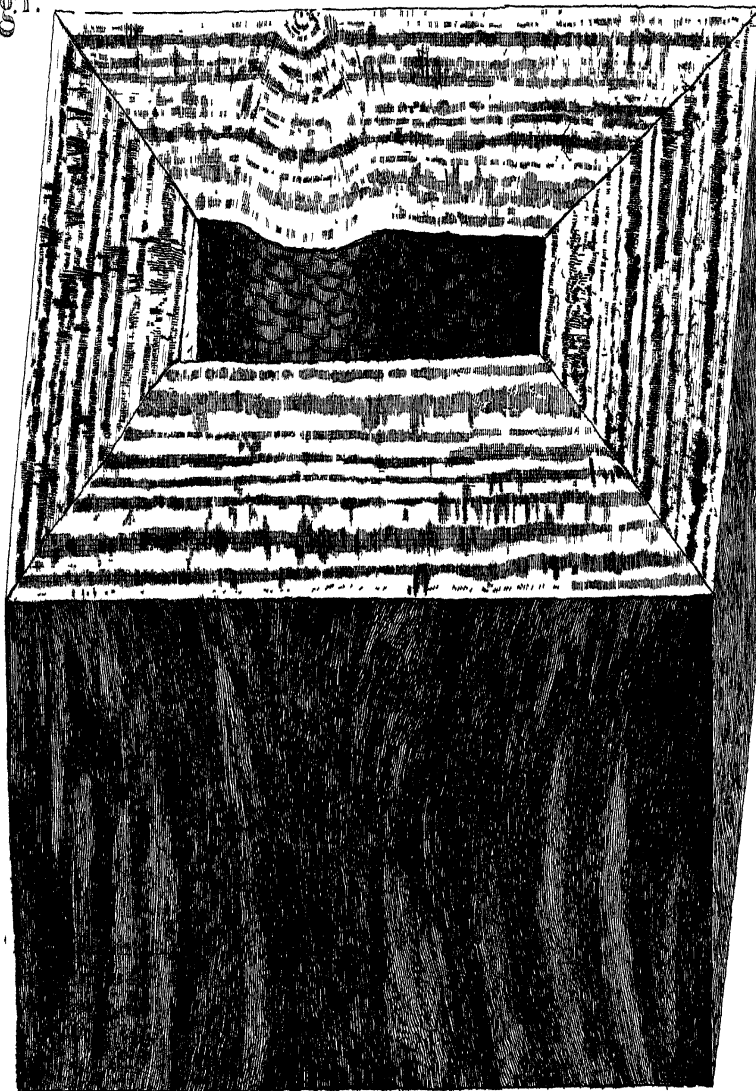
William Paradise.

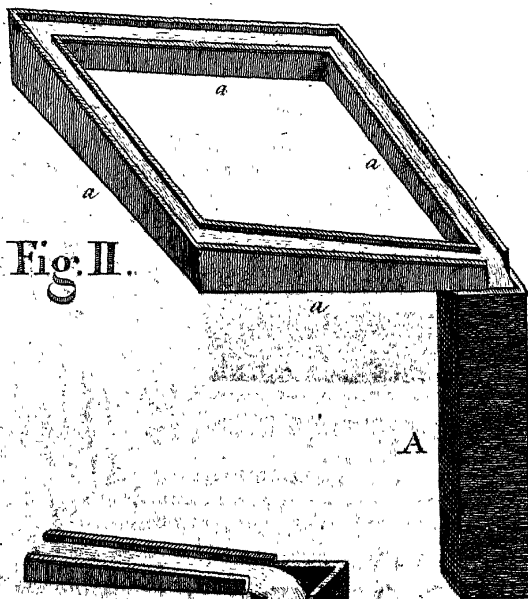
Holt, August 22, 1772.

XXX. A

B

Fig. 1.





XXX. *A Letter to Mathew Maty, M. D.
Sec. R. S.; containing some Observations
on a singular Sparry Incrustation found
in Somersetshire. By Edward King,
Esquire, F. R. S.*

John Street, Dec. 18, 1772.

S I R,

Read March 25,
1773. **T**H E summer before last, when I
was in Somersetshire, I received
a present of a very curious fossil from the Rev.
Mr. Catcott of Bristol, a gentleman who has taken
much pains to collect the fossil bodies of that
County, and is very indefatigable in his researches.
And as some few singular observations occur to
me with regard to this extraordinary mass, I take
the liberty to trouble you with them; and re-
quest, if you think them at all deserving any con-
sideration, that they may be laid before the Society,
together with a drawing of the fossil, (Tab. X.)
and with the very exact account I received of its
production from Mr. Catcott.

In the parish of High Littleton, in the County of
Somerset, situated about midway between Bristol
and Wells, are several Coal-mines; and about the
end of the year 1766, a new shaft (or pit) was
opened.

for the purpose of conveying air into an adjoining work, called Mearn's coal-pit; but when this shaft was finished, the water that flowed in from the sides, and which at first was taken up by buckets, greatly incommoded the under-works; and therefore, to prevent this inconvenience, the miners, at about the depth of ten fathom, and just below the place where the water broke in, affixed to the four sides of the pit some wooden shoots (as represented at Fig. 2.), about four or five inches wide, and as many deep; all of them a little inclined towards one corner, where was a hollow perpendicular pipe or trunk of elm, nearly a long square (as represented at A), being about seven inches and an half one way, and four inches and an half the other; and through this the water, that fell into the lateral shoots, was conveyed down to the level (or passage out); which being about seven fathoms lower than the shoots, the hollow perpendicular trunk was about fourteen yards in length.

This trunk having been thus fixed up, in the latter end of the year 1766, was in about three years time, or rather less, found to be much obstructed, and stopped up; so that, in August 1769, the miners were obliged to take it up: and then, on examining it, and taking it to pieces, they found the whole cavity, from one end to the other, nearly filled with a sort of sparry incrustation, somewhat softer than marble, but harder than alabaster, and which therefore I shall venture to call a species of marble. And the specimen now laid before the Society, and represented by the drawing (Fig. 1.), and also another specimen of the like sort, presented to the Society,

are merely transverse sections of the substance, with which the pipe was filled.

The water, that flowed into the pit on all sides, issued from a stratum of hard brown and reddish sand-stone, replete with shining sparry *micæ*, and some ocherous matter; and had, in its passage through the trunk, regularly filled up the cavity, by slow degrees, with solid incrustations; insomuch, that the increase of the marble is marked much in the same manner as the increase of the growth of a tree appears to be, when the trunk of it is cut horizontally: and at last the water had left only the cavity, which now appears in the middle of the block, and which was uniform in its figure from one end of the pipe to the other, and nearly similar to the original cavity; but which, at last, not being large enough to let all the water pass, occasioned the discovery. Since that time, in order to prevent the inconvenience (if possible) a new trunk has been made, larger than the first; and yet, in June 1771, this new trunk also was so far filled up with the sparry incrustation, that there was but just room to thrust four fingers into the central cavity; and the lateral shoots, or troughs, also have filled so fast, that they have been obliged, every now and then, to clean them out.

This, Sir, is the history of the specimen now laid before the Society, and of the Mine from whence it came; and the observations I would beg leave to make, are the following.

1st, As the water flowed in from the shoots, on two sides of the square trunk or pipe, it is manifest that the streams must have stricken against each

other, at the corner of the pipe where they first met, and also at the opposite corner. And, as it is a known principle of mechanics, that a body, which is acted upon by two forces moving in different directions, will describe the diagonal of a parallelogram, of which the directions of those forces shall be the sides; so here, the line in which the two streams met, and impeded each other's motion, has plainly, as the marble increased, gone on in the diagonal of such a parallelogram from both the corners; viz. from that where the pipe joined the shoots, or troughs, and from the opposite one: but it is also very remarkable, that there is such a diagonal line, not only at these corners, but in like manner at the other two; which can be accounted for no otherwise, than by supposing that each of the two streams, dashing against the opposite side of the pipe, formed continually, the whole way down, another stream, in a contrary direction, as represented in Fig. 3; and so, both together, produced the same effect throughout the whole pipe, as if there had been four streams flowing over the four sides. Upon examining the block, however, very strictly, it appears, that the lines in the diagonal one way, are stronger than those in the diagonal the other way; and indeed the specimen of the pipe, presented to the Society, has even broken in halves, exactly in one of the diagonals, though the block here described remains entire, and has the appearance of having had its sides joined accurately, in the manner in which a skilful workman would fit four boards to glued together.

2dly, At the place marked B, Fig. 1, there seems to have been, by some accident or other, the point of a small nail projecting into the pipe; and here, it is very remarkable, that, either by the dashing of the water, or rather perhaps by an effect which iron has been observed to have of hastening and increasing petrification, the incrustation has gone on faster than in other parts of the same side; but so regularly, that, from the point of the nail to the inner cavity, there is a swelling, or protuberance, so uniform, that it makes throughout nearly the same segment of different circles, of which the point of the nail is the common center; and that not merely directly opposite to the nail, but throughout this whole block and even further downwards.

3dly, The regular increase of these segments of circles is visible in each *lamina* of the block (if I may be allowed that expression), and in each *lamina* the diameter of the circle increases in due proportion; so that it is still nearly the same segment; though, if there be any difference, it is rather a smaller portion of a larger circle; as, from the cause which occasioned it, one would be led to expect. And with regard to these *laminæ*, it is worth observing, that as they mark the increase of the marble uniformly all round, as the growth of a tree is marked (only the marble increased inward, whereas a tree grows outward) so they seem to have become visible, and to have been thus distinctly marked, by means of the water bringing, at different times, more or less oker along with the sparry matter: and this is the more probable, as the whole country all round abounds

with beds of oker, and the waters are sometimes much tinged with it.

4thly, The cavity left in the middle of the block is not perfectly similar to the original cavity of the trunk, or pipe; because the water did not flow quite uniformly over the edges, at the ends of the shoots or troughs, in consequence, probably, of their not lying exactly horizontally: whence, more water fell upon and against one part of the sides of the trunk, than against the other.

5thly, The outside of the block has taken off impressions of all the roughnesses, knots, and shivers of the elm boards, which composed the trunk or pipe, even more accurately than they could have been taken off by wax, plaister of Paris, or almost any composition whatever, and certainly much more durably: which impressions, although they are not so well represented in the drawing, in consequence of their fineness, yet appear sufficiently plain, both on the specimen here described, and on that presented to the Society, and are exceedingly well deserving of notice.

There is in the Philosophical Transactions (Vol. LX. p. 47.) a very curious paper, from our learned foreign Member R. E. Raspe, concerning the production of white marble in a similar manner; in which paper he mentions the taking off impressions of medallions, by means of petrifying waters. And I remember a paper was read at the Royal Society some time ago, containing an account of several impressions, actually so taken off in a short time, in durable marble, by means of a petrifying water, near Bologna in Italy: when some of the impressions were also sent, both to the

the Royal Society, and to the British Museum. And, as this block here described, and the whole contents of the pipe, of above forty feet in length, were formed in less than three years, there is reason to conclude, that the water of this Mine in Somersetshire is as capable of being improved to the purposes of a new manufactory, as either that near Bologna, or those of Germany and Bohemia. And it is perhaps worth mentioning, that something of this sort has actually been attempted, with good success, in Peru; for we are told by P. Feuillée (who made several curious observations in South America, both physiological and astronomical, in 1709), that he saw many statues and beautiful vases (or holy water pots), in the churches at Lima, which were simply cast in moulds, by means alone of a petrifying water near Guankabalika, or Guankavelika. And this circumstance is also mentioned (p. 236.), in a Description of Peru, published in 1748, a great part of which is taken from Feuillée's account.

6thly, This block of marble takes a very fine polish, as appears by the specimen, the sections of which are polished; and if casts of medals, or other things, were taken in smooth moulds, well formed, their surfaces would, therefore, probably appear well polished, as those of the medals did, which came from Bologna.

7thly and lastly, I would only add, that Dr. Pococke, in his Travels (Vol. II. p. 264.), describing a very curious grotto in the island of Candia, or Crete, which exceeded all others that he ever saw in beauty, and the slenderness of the pillars, one of which is near twenty feet high, and even transparent, says,

says, “ As I had seen stones of this kind hewn out
 “ of a rock at Mount Lebanon, which were used as
 “ white marble, and appeared to be alabaster, this
 “ made me imagine, that when these sorts of petri-
 “ factions are hard enough to receive a polish, they
 “ then become the oriental transparent alabaster,
 “ which is so much valued, and of which there are
 “ two curious columns at the high altar of St. Mark
 “ in Venice.” Perhaps Dr. Pococke does not here
 sufficiently distinguish between marble and alabaster;
 but I add his remark, merely to shew how valuable
 these incrustations may become, and how much they
 deserve not to be neglected.

I am,

S I R,

Very respectfully,

Your most obedient,

humble servant,

Edward King.

XXXI. *Experiments and Observations on the Singing of Birds, by the Hon. Daines Barrington, Vice Pres. R. S. In a Letter to Mathew Maty, M. D. Sec. R. S.*

Read April 22, May 6, and May 13, 1773.

January 10, 1773.

DEAR SIR,

AS the experiments and observations I mean to lay before the Royal Society relate to the singing of birds, which is a subject that hath never before been scientifically treated of *, it may not be improper to prefix an explanation of some uncommon terms, which I shall be obliged to use, as well as others which I have been under a necessity of coining.

To *chirp*, is the first sound which a young bird utters, as a cry for food, and is different in all nestlings, if accurately attended to ; so that the hearer may distinguish of what species the birds are, though the nest may hang out of his sight and reach.

* Kircher, indeed, in his *Musurgia*, hath given us some few passages in the song of the nightingale, as well as the call of a quail and cuckow, which he hath engraved in musical characters. These instances, however, only prove that some birds have in their song, notes which correspond with the intervals of our common scale of the musical octave.

This

This cry is, as might be expected, very weak and querulous; it is dropped entirely as the bird grows stronger, nor is afterwards intermixed with its song, the *chirp* of a nightingale (for example) being hoarse and disagreeable.

To this definition of the *chirp*, I must add, that it consists of a single sound, repeated at very short intervals, and that it is common to nestlings of both sexes.

The *call* of a bird, is that sound which it is able to make, when about a month old; it is, in most instances (which I happen to recollect), a repetition of one and the same note, is retained by the bird as long as it lives, and is common, generally, to both the cock and hen*.

The next stage in the notes of a bird is termed, by the bird-catchers, *recording*, which word is probably derived from a musical instrument, formerly used in England, called a recorder†.

This attempt in the nestling to sing, may be compared to the imperfect endeavour in a child to babble. I have known instances of birds beginning to *record* when they were not a month old.

* For want of terms to distinguish the notes of birds, Bellon applies the verb *chantent*, or sing, to the goose and crane, as well as the nightingale. "Plusieurs oiseaux *chantent* la nuit, comme " est l'oye, la grue, & le rossignol." Bellon's Hist. of Birds, p. 50.

† It seems to have been a species of flute, and was probably used to teach young birds to pipe tunes.

Lord Bacon describes this instrument to have been strait, to have had a lesser and greater bore, both above and below, to have required very little breath from the blower, and to have had what he calls a *fipple*, or stopper. See his second Century of Experiments.

This

This first essay does not seem to have the least rudiments of the future song; but as the bird grows older and stronger, one may begin to perceive what the nestling is aiming at.

Whilst the scholar is thus endeavouring to form his song, when he is once sure of a passage, he commonly raises his tone, which he drops again when he is not equal to what he is attempting; just as a singer raises his voice, when he not only recollects certain parts of a tune with precision, but knows that he can execute them.

What the nestling is not thus thoroughly master of, he hurries over, lowering his tone, as if he did not wish to be heard, and could not yet satisfy himself.

I have never happened to meet with a passage in any writer, which seems to relate to this stage of singing in a bird, except, perhaps, in the following lines of Statius :

“ ———Nunc volucrum novi
 “ Questus, inexpertumque carmen,
 “ Quod tacitâ statuere brumâ.”

Stat. Sylv. L. iv. Ecl. 5.

A young bird commonly continues to *record* for ten or eleven months, when he is able to execute every part of his song, which afterwards continues fixed, and is scarcely ever altered.

When the bird is thus become perfect in his lesson, he is said to *sing his song round*, or in all its varieties of passages, which he connects together, and executes without a pause.

I would therefore define a bird's *song* to be a succession of three or more different notes, which are continued without interruption during the same interval with a musical bar of four crotchets in an adagio movement, or whilst a pendulum swings four seconds.

By the first requisite in this definition, I mean to exclude the call of a cuckow, or *clucking* of a hen *, as they consist of only two notes ; whilst the short bursts of singing birds, contending with each other (called *jerks* by the bird-catchers), are equally distinguished from what I term *song*, by their not continuing for four seconds.

As the notes of a cuckow and hen, therefore, though they exceed what I have defined the *call* of a bird to be, do not amount to its *song*, I will, for this reason, take the liberty of terming such a succession of two notes as we hear in these birds, the *varied call*.

Having thus settled the meaning of certain words, which I shall be obliged to make use of, I shall now proceed to state some general principles with regard to the singing of birds, which seem to result from the experiments I have been making for several years, and under a great variety of circumstances.

Notes in birds are no more innate, than language is in man, and depend entirely upon the master under which they are bred, as far as their organs will enable them to imitate the sounds which they have frequent opportunities of hearing.

* The common hen, when she lays, repeats the same note very often, and concludes with the sixth above, which she holds for a longer time.

Most of the experiments I have made on this subject have been made with cock linnets, which were fledged and nearly able to leave their nest, on account not only of this bird's docility, and great powers of imitation, but because the cock is easily distinguished from the hen at that early period, by the superior whiteness in the wing *.

In many other sorts of singing birds the male is not at the age of three weeks so certainly known from the female; and if the pupil turns out to be a hen,

“ ——— ibi omnis

“ Effusus labor.”

The Greek poets made a songster of the *τετλιξ*, whatever animal that may be, and it is remarkable that they observed the female was incapable of singing as well as hen birds :

Εἰτ' εἰσιν οἱ τετλιγες καὶ εὐδαιμονες,

ὣν ταις γυναιξίν αὖ δότιον φωνῆς ἐνι;

Comicorum Græcorum Sententiæ, p. 452.

Ed. Steph.

I have indeed known an instance or two of a hen's making out something like the song of her species; but these are as rare as the common hen's being heard to crow.

I rather suspect also, that those parrots, magpies, &c. which either do not speak at all, or very little, are hens of those species.

* The white reaches almost to the shaft of the quill feathers, and in the hen does not exceed more than half.

I have educated nestling linnets under the three best singing larks, the *skylark*, *woodlark*, and *titlark*, every one of which, instead of the linnet's song, adhered entirely to that of their respective instructors.

When the note of the *titlark-linnet* * was thoroughly *fixed*, I hung the bird in a room with two common linnets, for a quarter of a year, which were full in song; the *titlark-linnet*, however, did not borrow any passages from the linnet's song, but adhered stedfastly to that of the titlark.

I had some curiosity to find out whether an European nestling would equally learn the note of an African bird: I therefore educated a young linnet under a *vengolina* †, which imitated its African master so exactly, without any mixture of the linnet song, that it was impossible to distinguish the one from the other.

This *vengolina-linnet* was absolutely perfect, without ever uttering a single note by which it could have been known to be a linnet. In some of my other experiments, however, the nestling linnet retained the *call* of its own species, or what the bird-

* I thus call a bird which sings notes he would not have learned in a wild state; thus by a *skylark-linnet*, I mean a linnet with the skylark song; a *nightingale-robin*, a robin with the nightingale song, &c.

† This bird seems not to have been described by any of the ornithologists; it is of the *finch* tribe, and about the same size with our aberdavine (or fiskin). The colours are grey and white, and the cock hath a bright yellow spot upon the rump. It is a very familiar bird, and sings better than any of those which are not European, except the American *mocking bird*.

catchers term the linnet's *chuckle*, from some resemblance to that word when pronounced.

I have before stated, that all my nestling linnets were three weeks old, when taken from the nest; and by that time they frequently learn their *own call* from the parent birds, which I have mentioned to consist of only a single note.

To be certain, therefore, that a nestling will not have even the *call* of its species, it should be taken from the nest when only a day or two old; because, though nestlings cannot see till the seventh day, yet they can hear from the instant they are hatched, and probably, from that circumstance, attend to sounds, more than they do afterwards, especially as the call of the parents announces the arrival of their food.

I must own, that I am not equal myself, nor can I procure any person to take the trouble of breeding up a bird of this age, as the odds against its being reared are almost infinite. The warmth indeed of incubation may be, in some measure, supplied by cotton and fires; but these delicate animals require, in this state, being fed almost perpetually, whilst the nourishment they receive should not only be prepared with great attention, but given in very small portions at a time.

Though I must admit, therefore, that I have never reared myself a bird of so tender an age, yet I have happened to see both a linnet and a goldfinch which were taken from their nests when only two or three days old.

The first of these belonged to Mr. Matthews, an apothecary at Kenfington, which, from a want of
other

other sounds to imitate, almost articulated the words *pretty boy*, as well as some other short sentences: I heard the bird myself repeat the words *pretty boy*; and Mr. Matthews assured me, that he had neither the note or call of any bird whatsoever.

This talking linnet died last year, and many people went from London to hear him speak.

The goldfinch I have before mentioned, was reared in the town of Knighton in Radnorshire, which I happened to hear, as I was walking by the house where it was kept.

I thought indeed that a *wren* was singing; and I went into the house to inquire after it, as that little bird seldom lives long in a cage.

The people of the house, however, told me, that they had no bird but a goldfinch, which they conceived to sing its own natural note, as they called it; upon which I staid a considerable time in the room, whilst its notes were merely those of a *wren*, without the least mixture of goldfinch.

On further inquiries, I found that the bird had been taken from the nest when only two or three days old, that it was hung in a window which was opposite to a small garden, whence the nestling had undoubtedly acquired the notes of the wren, without having had any opportunity of learning even the *call* of the goldfinch.

These facts which I have stated seem to prove very decisively, that birds have not any innate ideas of the notes which are supposed to be peculiar to each species. But it will possibly be asked, why in a wild state they adhere so steadily to the same song,
in

in so much that it is well known, before the bird is heard, what notes you are to expect from him.

This, however, arises entirely from the nestling's attending only to the instruction of the parent bird, whilst it disregards the notes of all others, which may perhaps be singing round him.

Young Canary-birds are frequently reared in a room where there are many other sorts; and yet I have been informed that they only learn the song of the parent cock.

Every one knows, that the common house-sparrow, when in a wild state, never does any thing but chirp: this, however, does not arise from want of powers in this bird to imitate others; but because he only attends to the parental note.

But, to prove this decisively, I took a common Sparrow from the nest when it was fledged, and educated him under a linnet: the bird, however, by accident heard a goldfinch also, and his song was, therefore, a mixture of the linnet and goldfinch.

I have tried several experiments, in order to observe from what circumstances birds fix upon any particular note when taken from the parents; but cannot settle this with any sort of precision, any more than at what period of their *rearing* they determine upon the song to which they will adhere.

I educated a young robin under a very fine Nightingale; which, however, began already to let out of song, and was perfectly mute in less than a fortnight.

This robin afterwards sung three parts in four *nightingale*; and the rest of his song was what the
bird.

bird-catchers call *rubbish*, or no particular note whatsoever.

I hung this robin nearer to the nightingale than to any other bird; from which first experiment I conceived, that the scholar would imitate the master which was at the least distance from him.

From several other experiments, however, which I have since tried, I find it to be very uncertain what notes the nestling will most attend to, and often their song is a mixture; as in the instance which I before stated of the sparrow.

I must own also, that I conceived, from the experiment of educating the robin under a nightingale, that the scholar would fix upon the note which it first heard when taken from the nest; I imagined likewise, that, if the nightingale had been fully in song, the instruction for a fortnight would have been sufficient.

I have, however, since tried the following experiment, which convinces me, so much depends upon circumstances, and perhaps caprice in the scholar, that no general inference, or rule, can be laid down with regard to either of these suppositions.

I educated a nestling robin under a woodlark-linnet, which was full in song, and hung very near to him for a month together: after which, the robin was removed to another house, where he could only hear a skylark-linnet. The consequence was that the nestling did not sing a note of woodlark (though I afterwards hung him again just above the wood-lark-linnet) but adhered entirely to the song of the skylark-linnet.

Having

Having thus stated the result of several experiments, which were chiefly intended to determine, whether birds had any innate ideas of the notes, or song, which is supposed to be peculiar to each species, I shall now make some general observations on their singing; though perhaps the subject may appear to many a very minute one.

Every poet, indeed, speaks with raptures of the harmony of the groves; yet those even, who have good musical ears, seem to pay little attention to it, but as a pleasing noise.

I am also convinced (though it may seem rather paradoxical), that the inhabitants of London distinguish more accurately, and know more on this head, than of all the other parts of the island taken together.

This seems to arise from two causes.

The first is, that we have not more musical ideas which are innate, than we have of language; and therefore those even, who have the happiness to have organs which are capable of receiving a gratification from this sixth sense (as it hath been called by some) require, however, the best instruction.

The orchestra of the opera, which is confined to the metropolis, hath diffused a good stile of playing over the other bands of the capital, which is, by degrees, communicated to the fidler and ballad-finger in the streets; the organs in every church, as well as those of the Savoyards, contribute likewise to this improvement of musical faculties in the Londoners.

If the singing of the ploughman in the country is therefore compared with that of the London black-

guard, the superiority is infinitely on the side of the latter ; and the same may be observed in comparing the voice of a country girl and London house-maid, as it is very uncommon to hear the former sing tolerably in tune.

I do not mean by this, to assert that the inhabitants of the country are not born with as good musical organs ; but only, that they have not the same opportunities of learning from others, who play in tune themselves.

The other reason for the inhabitants of London judging better in relation to the song of birds, arises from their hearing each bird sing distinctly, either in their own or their neighbours shops ; as also from a bird continuing much longer in song whilst in a cage, than when at liberty ; the cause of which I shall endeavour hereafter to explain.

Those who live in the country, on the other hand, do not hear birds sing in their woods for above two months in the year, when the confusion of notes prevents their attending to the song of any particular bird ; nor does he continue long enough in a place, for the hearer to recollect his notes with accuracy.

Besides this, birds in the spring sing very loud indeed ; but they only give short jerks, and scarcely ever the whole compass of their song.

For these reasons, I have never happened to meet with any person, who had not resided in London, whose judgement or opinion on this subject I could the least rely upon ; and a stronger proof of this cannot be given, than that most people, who keep Ca-
nary

nary-birds do not know that they sing chiefly either the titlark, or nightingale notes*.

Nothing, however, can be more marked than the note of a nightingale called its *jug*, which most of the Canary-birds brought from the Tyrol commonly have, as well as several nightingale *strokes*, or particular passages in the song of that bird.

I mention this superior knowledge in the inhabitants of the capital, because I am convinced, that, if others are consulted in relation to the singing of birds, they will only mislead, instead of giving any material or useful information†.

Birds in a wild state do not commonly sing above ten weeks in the year; which is then also confined to the cocks of a few species; I conceive, that this last

* I once saw two of these birds which came from the Canary islands; neither of which had any song at all; and I have been informed, that a ship brought a great many of them not long since, which sung as little.

Most of those Canary-birds, which are imported from the Tyrol, have been educated by parents, the progenitor of which was instructed by a nightingale; our English Canary-birds have commonly more of the titlark note.

The traffick in these birds makes a small article of commerce, as four Tyroleze generally bring over to England sixteen hundred every year; and though they carry them on their backs one thousand miles, as well as pay 20l. duty for such a number, yet upon the whole it answers to sell these birds at 5s. a piece.

The chief place for breeding Canary-birds is Inspruck and its environs, from whence they are sent to Constantinople, as well as every part of Europe.

† As it will not answer to catch birds with clap-nets any where but in the neighbourhood of London, most of the birds which may be heard in a country town are nestlings, and consequently cannot sing the supposed natural song in any perfection.

circumstance arises from the superior strength of the muscles of the larynx.

I procured a cock nightingale, a cock and hen blackbird, a cock and hen rook, a cock linnet, as also a cock and hen chaffinch, which that very eminent anatomist, Mr. Hunter, F. R. S. was so obliging as to dissect for me, and begged, that he would particularly attend to the state of the organs in the different birds, which might be supposed to contribute to singing.

Mr. Hunter found the muscles of the larynx to be stronger in the nightingale than in any other bird of the same size; and in all those instances (where he dissected both cock and hen) that the same muscles were stronger in the cock.

I sent the cock and hen rook, in order to see whether there would be the same difference in the cock and hen of a species which did not sing at all. Mr. Hunter, however, told me, that he had not attended so much to their comparative organs of voice, as in the other kinds; but that, to the best of his recollection, there was no difference at all.

Strength, however, in these muscles, seems not to be the only requisite; the birds must have also great plenty of food, which seems to be proved sufficiently by birds in a cage singing the greatest part of the year, when the wild ones do not (as I observed before) continue in song above ten weeks.

The food of singing birds consists of plants, insects, or seeds, and of the two first of these there is infinitely the greatest profusion in the spring.

As for seeds, which are to be met with only in the autumn, I think they cannot well find any great quantities

quantities of them in a country so cultivated as England is; for the seeds in meadows are destroyed by mowing; in pastures, by the bite of the cattle; and in arable, by the plough, when most of them are buried too deep for the bird to reach them*.

I know well that the singing of the cock-bird in the spring is attributed by many† to the motive only of pleasing its mate during incubation.

Those, however, who suppose this, should recollect, that much the greater part of birds do not sing at all: why should their mate therefore be deprived of this solace and amusement?

The bird in a cage, which, perhaps, sings nine or ten months in a year, cannot do so from this inducement; and, on the contrary, it arises chiefly from contending with another bird, or indeed against almost any sort of continued noise.

Superiority in song gives to birds a most amazing ascendancy over each other; as is well known to the bird-catchers by the fascinating power of their call-birds, which they contrive should moult prematurely for this purpose.

But, to shew decisively that the singing of a bird in the spring does not arise from any attention to its mate, a very experienced catcher of nightingales hath informed me, that some of these birds have *jerked* the instant they were caught. He hath also brought

* The plough indeed may turn up some few seeds, which may still be in an eatable state.

† See, amongst others, M. de Buffon, in his lately-published Ornithology.

to me a nightingale, which had been but a few hours in a cage, and which burst forth in a roar of song.

At the same time this bird is so sulky on its first confinement, that he must be crammed for seven or eight days, as he will otherwise not feed himself: it is also necessary to tie his wings, to prevent his killing himself against the top or sides of the cage.

I believe there is no instance of any bird's singing which exceeds our blackbird in size; and possibly this may arise from the difficulty of its concealing itself, if it called the attention of its enemies, not only by bulk, but by the proportionable loudness of its notes*.

I should rather conceive, it is for the same reason that no hen-bird sings, because this talent would be still more dangerous during incubation; which may possibly also account for the inferiority in point of plumage.

I shall now consider how far the singing of birds resembles our known musical intervals, which are never marked more minutely than to half notes; because, though we can form every gradation from half-note to half-note, by drawing the finger gently over the string of a violin, or covering by degrees the hole of a flute; yet we cannot produce such a minute interval at command, when a quarter-note for example might be required.

Ligon, indeed, in his history of Barbadoes, hath the following passage: "The next bird is of the co-

* For the same reason, most large birds are wilder than the smaller ones.

“lour of the fieldfare; but the head is too large
 “for the body; and for that reason she is called a
 “counsellor. She performs that with her voice,
 “which no instrument can play, or voice can sing;
 “and that is quarter-notes, her song being composed
 “of them, and every one a note higher than another.”
 Hist. Barb. p. 60.

Ligon appears, from o’her parts of his work, to have been musical; but I should doubt much whether he was quite sure of these quarter intervals, so as to speak of them with precision.

Some passages of the song in a few kinds of birds correspond with the intervals of our musical scale (of which the cuckow is a striking and known instance): much the greater part, however, of such song is not capable of musical notations.

This arises from three causes: the first is, that the rapidity is often so great, and it is also so uncertain when they may stop, that we cannot reduce the passages to form a musical bar, in any time whatsoever.

The second is, that the pitch of most birds is considerably higher * than the most shrill notes of those instruments which contain even the greatest compass.

* Dr Wallis is mistaken in part of what he supposes to be the cause of shrillness in the voice, “*Nam ut tubus, sic trachea longior, & strictior, sonum efficit magis acutum.*” Grammar, p. 3.

The narrower the pipe is, the more sharp the pitch as he rightly observes; but the length of the tube hath just the contrary effect, because players on the flute always insert a longer middle-piece, when they want to make the instrument more flat.

I have

I have before said, that our ideas of a voice, or instrument, being perfectly in tune or not, arise from comparing it with the musical intervals to which we are most accustomed.

As the upper and lower parts of every instrument, however, are but seldom used, we are not so well acquainted with the intervals in the highest and lowest octaves, as we are with those which are more central; and for this reason the harpsichord-tuners find it more difficult to tune these extreme parts.

As a bird's pitch, therefore, is higher than that of any instrument, we are consequently at a still greater loss when we attempt to mark their notes in musical characters, which we can so readily apply to such as we can distinguish with precision.

The third, however, and unsurmountable difficulty is, that the intervals used by birds are commonly so minute, that we cannot judge at all of them from the more gross intervals into which we divide our musical octave.

It should therefore be recollected, by those who have contended that the Greeks and Romans were acquainted with such more minute intervals of the octave, that they must insist the ancients had organs of sensation, with which their degenerate posterity are totally unprovided.

Though we cannot attain the more delicate and imperceptible intervals in the song of birds*, yet many of them are capable of whistling tunes with our more gross intervals, as is well known by the

* There have been instances indeed of persons who would whistle the notes of birds, but these are too rare to be argued from.

common instances of piping Bullfinches *, and Canary-birds.

This, however, arises from mere imitation of what they hear when taken early from the nest; for if the instrument from which they learn is out of tune, they as readily pipe the false, as the true notes of the composition.

The next point of comparison to be made between our music and that of birds is, whether they always sing in the same pitch.

This, however, I will not presume to answer with any precision, for the reason I have before suggested; I shall, however, without reserve, give the best conjectures I can form on this head.

If a dozen singing birds of different kinds are heard in the same room, there is not any disagreeable dissonance (which is not properly resolved), either to my own ear, or to that of others, whose judgement on such a point I can more rely.

At the same time, as each bird is singing a different song, it is extraordinary that what we call harmony should not be perpetually violated, as we experience, in what is commonly called a Dutch concert, when several tunes are played together.

The first requisite to make such sounds agreeable to the ear is, that all the birds should sing in the same key, which I am induced to believe that they do, from the following reasons.

I have long attended to the singing of birds, but if I cannot have recourse to an instrument very soon,

* These Bullfinches also form a small article of commerce, and are chiefly brought from the neighbourhood of Cologne.

I cannot carry the pitch of their notes in my memory, even for a very short time.

I therefore desired a very experienced harpsichord-tuner (who told me he could recollect any particular note which he happened to hear for several hours), to mark down when he returned home what he had observed on this head.

I have lately received an account from him of the following notes in different birds.

F. natural in woodlarks.

A. natural in common cocks.

C. natural in Bantam cocks.

B. flat in a very large cock.

C. falling to A. commonly in the cuckow.

A. in thrushes.

D. in some owls.

B. flat in some others.

These observations furnish five notes, viz. A. B. flat, C. D. and F. to which I can add a sixth, (viz. G.) from my own observations on a nightingale which lived three years in a cage. I can also confirm these remarks of the harpsichord-tuner by having frequently heard from the same bird C. and F.

As one should speak of the pitch of these notes with some precision, the B. flat of the spinnet I tried them by, was perfectly in tune with the great bell of St. Paul's.

The following notes, therefore, having been observed in different birds, viz. A. B. flat, C. D. F. and G. the E. is only wanting to compleat the scale; the six other notes, however, afford sufficient data for making some conjectures, at least, with regard to the key in which birds may be supposed to sing,

as these intervals can only be found in the key of F. with a sharp third, or that of G. with a flat third.

I must own, I should rather suppose it to be the latter, and for the following reasons.

Lucretius says (and perhaps the conjecture is not only ingenious but well founded), that the first musical notes were learned from birds :

“ At liquidas avium voces imitauer ore

“ Ante fuit multo, quam lævia carmina cantu

“ Concelebrare homines possent, cantuque juvare.”

Now, of all the musical tones which can be distinguished in birds, those of the cuckow have been most attended to, which form a flat third, not only by the observations of the harpsichord tuner I have before mentioned, but likewise by those of Kircher, in his *Musurgia*.

I know well that there have been some late compositions, which introduce the cuckow notes in a sharp third; these composers, however, did not trouble themselves with accuracy in imitating these notes, and it answered their purpose sufficiently, if there was a general resemblance.

Another proof of our musical intervals being originally borrowed from the song of birds, arises from most compositions being in a flat third, where music is simple, and consists merely of melody.

The oldest tune I happen to have heard is a Welsh one, called *Morvar Rhydland* *, which is

* Or *Rhydland Marsh*, where the Welsh received a great defeat; Rhydland is in Flintshire. We find also, by the *Orpheus Britannicus*, that even so late as the time of Purcell, two parts in the three of his compositions are in the flat third.

composed in a flat third; and if the music of the Turks and Chinese is examined in Du Halde and Dr. Shaw, half of the airs are also in a flat third.

The music of two centuries ago is likewise often in a flat third, though ninety-nine compositions out of a hundred are now in the sharp third.

The reason, however, of this alteration seems to be very clear: the flat third is plaintive, and consequently adapted to simple movements, such as may be expected in countries where music hath not been long cultivated.

There is on the other hand a most striking brilliancy in the sharp third, which is therefore proper for the amazing improvements in execution, which both singers and players have arrived at within the last fifty years.

When Corelli's music was first published, our ablest violinists conceived that it was too difficult to be performed; it is now, however, the first composition which is attempted by a scholar. Every year also now produces greater and greater prodigies upon other instruments, in point of execution.

I have before observed, that by attending to a nightingale, as well as a robin which was educated under him, I always found that the notes reducible to our intervals of the octave were precisely the same; which is another proof that birds sing always in the same key.

In this circumstance, they differ much from the human singer; because those who are not able to *sing at sight*, often begin a song either above or below the compass of their voice, which they are not therefore able to go through with. As birds, however, form the same passages with the same notes,

Compositions for two young Bullfrogs.

Allegretto

1st B.

2^d B.

1st B.

2^d B.

Allegro

1st B.

2^d B.

Allegro

1st B.

2^d B.

at all times, this mistake of the pitch can never happen in them.

Few fingers again can continue their own part, whilst the same passages are sung by another in a different key; or if the same or other passages are sung, so as not to coincide with the musical bar, or time of the first finger.

As birds however adhere so steadfastly to the same precise notes in the same passages, though they never trouble themselves about what is called *time* in music; it follows that a composition may be formed for two piping bulfinches, in two parts, so as to constitute true harmony, though either of the birds may happen to begin, or stop, when they please.

I have therefore procured such an ingenious composition, by a very able musician *, which I send herewith; and it need scarcely be observed, that there cannot possibly be much variety in the part of the second bulfinch. [See TAB. XI.]

Though several birds have great musical powers, yet they seem to have no delicacy of sensations, as the human finger hath; and therefore the very best of them cannot be taught to exceed the insipidity of the upper part of the flute stop of an organ †, which hath not the modern improvement of a *swell*.

* Mr. Zeidler, who plays the violincello at Covent Garden theatre.

† Lord Bacon mentions, that in the instrument called a *regall* (which was a species of portable organ), there was a *nightingale* stop, in which water was made use of to produce the stronger imitation of this bird's tone. See Cent. ii. exper. 172. Though this instrument, as well as its *Nightingale* stop, is now disused, I have procured an organ pipe to be immersed partly in water, which, when blown into, hath produced a tone very similar to that of birds.

They

They are also easily imposed upon by that most imperfect of all instruments, a *bird-call*, which they often mistake for the notes of their own species.

I have before observed, that perhaps no bird may be said to sing which is larger than a blackbird, though many of them are taught to speak: the smaller birds, however, have this power of imitation; though perhaps the larger ones have not organs which may enable them, on the other hand, to sing.

We have the following instances of birds being taught to speak, in the time of the Greeks and Romans, upon which we never try the same experiment. Moschus addresses nightingales and swallows which were thus instructed:

Ἀδονίδες, पासαι τε χελιδόνες, ας παρ' ἐέρπεν,
 Ας λαλεῖν ἐδίδασκε.

Moschi Idyl. iii.

Pliny mentions both a cock, thrush, and nightingales, which articulated *:

“Habebant & Cæsares juvenes turdum †, item
 “*lusciniæ* Græco atque Latino sermone dociles, præ-
 “terea meditantes in diem, & assidue nova lo-
 “quentes longiore etiam contextu.”

Statius also takes notice of some birds speaking, which we never attempt to teach in this manner:

* Lib. x. c. 21 & 42.

† Ibid. The other *turdus* belonged to the Empress Agrippina.

“ Huc doctæ stipentur aves, queis nobile fandi
 “ Jus natura dedit, plangat Phœbeius ales,
 “ Auditasque memor penitus demittere voces
 “ Sturnus, & Aonio versæ certamine picæ;
 “ Quique refert jungens iterata vocabula perdix,
 “ Et quæ Bistonio queritur soror orba cubili *.”

Stat. Sylv. lib. ii. ecl. 4.

* Amongst the five birds mentioned in these lines of Statius, there are four which are never taught to speak at present, viz. the cock, the nightingale, the common, and the *red legged partridge*.

As I suppose, however, that *perdix* signifies this last bird, and not the common partridge (as it is always translated), it is proper I should here give my reasons why I dissent from others, as also why I conceive that *sturnus*, in this passage, is not a *starling*, but the common partridge.

None of the ancients have described the plumage of the *perdix*; but Aristotle, Ovid, and Pliny, inform us of what materials the nest of this bird is composed, as well as where it is placed.

Aristotle says, that the nest is *fertified with wood*^a; and in another chapter^b, with *thorns and wood*; neither of which are used by the common partridge, which often builds in a country where they cannot be procured.

On the contrary, M. de Buffon informs us, that the red legged partridge, “ se tiennent sur les montagnes qui produisent beaucoup de buissons, & de broussailles^c.”

^a Επηλυγαζομεναι υλιν. Lib. v. c. 1. Which Stephens renders *making a covering of wood*.

^b Lib. ix. c. 8. The common partridge, however, makes its nest with hay and straw.

^c Orn. T. II. p. 433.

Silius Italicus also describes the *pernix* as being found in the same sort of country,

“ Ceu *pernix*, quam densa vagis latratibus implet

“ Venator *dumeta* Lacon.” Lib. iii. l. 29.

As we find, from these citations, that so many different sorts of birds have learned to speak, and as I have shewn that a sparrow may be taught to

Ovid, therefore, speaking of the *perdix*, says,

“ ——— ponitque in sepibus ova ^d,”

where the common partridge is seldom known to build.

Pliny again informs us, “ perdices spinâ & frutice sic muniant receptaculum, ut contra feras abunde valentur ^e”, as also in the 52d chapter of his tenth book, that the *perdix* lays white eggs, which is not true of the common partridge.

But there are not wanting other proofs of the conjecture I have here made.

Aristotle, speaking of this same bird, says, *Ἰσὼν μὲν περδικῶν, οἱ μὲν κακκαεῖζουσιν, οἱ δὲ πριεσι* ^f.

Now, the word *κακκαεῖζουσι* is clearly formed from the *call* of the bird alluded to, which does not at all resemble that of the common partridge.

Thus also the author of the Elegy on the Nightingale, who is supposed by some to be Ovid, hath the following line :

“ *Caccabat hinc perdix, hinc gratitat improbus anser.*”

so that the call of the bird must have had something very particular, and have answered nearly, to the words *κακκαεῖζει* and *caccabat*.

I find, indeed, that M. de Buffon contends ^g that the *περδική* of Aristotle does not mean the common partridge, but the bar-tavel, with regard to which, I shall not enter into any discussion, but only observe, that most of his references are inaccurate, and that he entirely mistakes the materials of which the nest is composed, according to Aristotle's sixth book, and first chapter.

^d Ovid. Met. lib. viii. l. 258. I shall also refer to l. 237, of the same book :

“ *Garrula ramosa prospexit ab ilice perdix :*”

as it is well known that the common partridge never perches upon a tree.

^e Lib. x. c. 23.

^f Lib. iv. c. 9.

^g Orn. T. II. p. 422.

sing the linnet's note, I scarcely know what species to fix upon, that may be considered as incapable of such imitations; for it is very clear, from several experiments before stated, that the utmost endeavours will not be wanting in the bird, if he is endowed with the proper organs.

It can therefore only be settled by educating a bird, under proper circumstances, whether he is thus qualified or not; for if one was only to determine this point by conjecture, one should suppose

If, indeed, M. de Buffon had not been thus inaccurate, the grass and leaves, of which he says it is made, would prove that Aristotle speaks of the common partridge, contrary to what he himself supposes.

But the strongest proof that *perdix* signifies the red legged partridge is, that the Italians to this day call this bird *pernice*^h, and the common sort *sarna*ⁱ.

This also now brings me to the proofs, of *sturnus* in this passage of Statius signifying the *common partridge*, and not the *starling*, which I must admit are not so strong as with regard to the import of the word *perdix*. If my arguments are not therefore so convincing on this head, the number of birds taught to speak by the Romans, and not by us, must be reduced to three, as the starling is frequently learned to talk in the present times.

As I cannot argue from the description of the habits of the *sturnus*, or the materials of its nest, as in the former instance, I must rest my conjecture (such as it is) on the two birds, almost following each other in these lines of Statius; on the common partridge being called *sarna* to this day by the Italians, and upon the Romans having had otherwise no name for our partridge (which is a very common bird in Italy), if *sturnus* is supposed to signify only a *starling*.

^h I cannot therefore but think that *pernix*, according to the reading in the passage of Silius Italicus, which I have before cited, is the true Latin name for this bird, and that it was called *perdix* by those only who understood Greek.

ⁱ See Olina.

that a sparrow would not imitate the song of the linnet, nor that a nightingale or partridge could be taught to speak.

And here it may not be improper to explain what I mean by birds learning to imitate the notes of others, or the human speech.

If the birds differ little in shape or size (particularly of the beak *), the imitation is commonly so strong, that

* It seems very obvious why the form and size of the beak may be material; but I have also observed, that the colour of a bird's bill changes, when in or out of song; and I am informed that a cock seldom crows much, but when his comb is red.

When most of the finch tribe are coming into song, there is such a gradual change in the colour of their bill; thus, those of the chaffinch and linnet are then of a very deep blue, which fades away again, when the bird ceases to be in song.

This particular should be attended to by the ornithologist, in his description; because otherwise he supposes the colour of the bill to be permanent, which is by no means so.

This alteration, however, rather seems to be the symptom than the cause of a bird's coming into song, or otherwise, and I have never attended to this circumstance in the soft billed birds sufficiently, to say whether it holds also with regard to them.

A very intelligent bird-catcher, however, was able to prognosticate, for three winters together, when a nightingale, which I kept so long, was coming into song (though there was no change in the colour of the bill), by the dung's being intermixed with large bloody spots, which before was only of a dead white.

This same bird-catcher was also very successful in his prescriptions for sick birds, with regard to the ingredients of which he was indeed very mysterious.

He said, that as he could not feel their pulse, the circumstances which he chiefly attended to were their weight, as well as both the consistence and colour of their dung.

He always frankly said what he expected from his prescriptions, and that if such and such changes did not soon take place,
" Miré

“ *Mirè sagaces falleret hospites*

“ *Dilcrimen obſcurum.*”

Horat.

for, in ſuch inſtances, the paſſages are not only the ſame, but the tone.

Such was the event of the experiment I have before mentioned of the linnet educated under a vengolina.

In my experiment, however, of teaching the ſparrow the notes of the linnet, though the ſcholar imitated the paſſages of its maſter, yet the tone of the ſparrow had by no means the mellowneſs of the original.

The imitation might therefore be, in ſome meaſure, compared to the ſinging of an opera ſong by a black-guard, when, though the notes may be preſiſely the ſame, yet the manner and tone would differ very much.

Thus alſo the linnet, which I heard repeat the words *pretty boy*, did not articulate like a *parrot* or a *mino*, though, at the ſame time, the words might be clearly diſtinguiſhed.

The education I have therefore been ſpeaking of will not give new organs of voice to a bird, and the inſtrument itſelf will not vary, though the notes or paſſages may be altered almoſt at pleaſure.

I tried once an experiment, which might indeed have poſſibly made ſome alteration in the tone of a bird, from what it might have been when the animal

the caſe was deſperate. He frequently alſo reſuſed to preſcribe, if the bird felt too light in the hand, or he thought that there was not ſufficient time to bring about an alteration in the dung.

was at its full growth, by procuring an operator who caponised a young blackbird of about six weeks old ; as it died, however, soon afterwards, and I have never repeated the experiment, I can only conjecture with regard to what might have been the consequences of it.

Both Pliny * and the London poulterers agree that a capon does not crow, which I should conceive to arise from the muscles of the larynx never acquiring the proper degree of strength, which seems to be requisite to the singing of a bird, from Mr. Hunter's dissections.

But it will perhaps be asked, why this operation should not improve the notes of a nestling, as much as it is supposed to contribute to the greater perfection of the human voice.

To this I answer, that castration by no means insures any such consequence ; for the voices of much the greater part of Italian eunuchs are so indifferent, that they have no means of procuring a livelihood but by copying music, and this is one of the reasons why so few compositions are published in Italy, as it would starve this refuse of society.

But it may be said, that there hath been a Farinelli and a Manzoli, whose voices were so distinguishedly superior.

To this I again answer, that the catalogue of such names would be a very short one ; and that we attribute those effects to castration, which should rather be ascribed to the education of these singers.

* Lib. x. c. 21.

Castration commonly leaves the human voice at the same pitch as when the operation is performed; but the eunuch, from that time, is educated with a view only to his future appearance on the opera stage; he therefore manages his voice to greater advantage, than those who have not so early and constant instruction.

Considering the size of many singing birds, it is rather amazing at what a distance their notes may be heard.

I think I may venture to say, that a nightingale may be very clearly distinguished at more than half a mile *, if the evening is calm. I have also observed the breath of a robin (which exerted itself) so condensed in a frosty morning, as to be very visible.

To make the comparison, however, with accuracy, between the loudness of a bird's and the human voice, a person should be sent to the spot from whence the bird is heard; I should rather conceive that, upon such trial, the nightingale would be distinguished further than the man.

It must have struck every one, that, in passing under a house where the windows are shut, the singing of a bird is easily heard, when, at the same time, a conversation cannot be so, though an animated one.

Most people, who have not attended to the notes of birds, suppose that those of every species sing

* Mons. de Buffon says, that the quadruped which he terms the *huarins*, may be heard at the distance of a league. Ornith. Tom. I.

exactly the same notes and passages, which is by no means true, though it is admitted that there is a general resemblance.

Thus the London bird-catchers prefer the song of the Kentish goldfinches, but Essex chaffinches; and when they sell the bird to those who can thus distinguish, inform the buyer that it hath such a note, which is very well understood between them*.

Some of the nightingale fanciers also prefer a Surry bird to those of Middlesex†.

These differences in the song of birds of the same species cannot perhaps be compared to any thing more apposite, than the varieties of provincial dialects.

The nightingale seems to have been fixed upon, almost universally, as the most capital of singing birds, which superiority it certainly may boldly challenge: one reason, however, of this bird's being

* These are the names which they give to some of the nightingale's notes: *Sweet, Sweet jug, Jug sweet, Water bubble, Pipe rattle, Bell pipe, Scroty, Skeg, Skeg, skeg, Swat swat swaty, Whitlow whitlow whitlow*, from some distant affinity to such words.

† Mr. Henshaw informs us, that nightingales in Denmark are not heard till May, and that their notes are not so sweet or various as with us. Dr. Birch's History of the Royal Society, Vol. III. p. 189. Whilst Mr. Fletcher (who was minister from Q. Elizabeth to Russia) says, that the nightingales in that part of the world have a finer note than ours. See Fletcher's Life, in the Biographia Britannica.

I never could believe what is commonly asserted, that the Czar Peter was at a considerable expence to introduce singing birds near Petersburg; because it appears, by the *Fauna Suecica*, that they have in those latitudes most of the same birds with those of England.

more attended to than others is, that it sings in the night *.

Hence Shakespeare says,

“ The nightingale, if she should sing by day,
 “ When every goose is cackling, would be thought
 “ No better a musician than the wren.”

The song of this bird hath been described, and expatiated upon, by several writers, particularly Pliny and Strada.

As I must own, however, that I cannot affix any precise ideas to either of these celebrated descriptions, and as I once kept a very fine bird of this sort for three years, with very particular attention to its song; I shall endeavour to do it the best justice I am capable of.

In the first place, its tone is infinitely more mellow than that of any other bird, though, at the same time, by a proper exertion of its musical powers, it can be excessively brilliant.

When this bird *sang its song round*, in its whole compass, I have observed sixteen different beginnings and closes, at the same time that the intermediate notes were commonly varied in their succession with such judgment, as to produce a most pleasing variety.

The bird which approaches nearest to the excellence of the nightingale, in this respect, is the skylark; but then the tone is infinitely inferior in point of mellowness: most other singing birds have not above four or five changes.

* The woodlark and reedsparrow sing likewise in the night; and from hence, in the neighbourhood of Shrewsbury, the latter hath obtained the name of the willow-nightingale. Nightingales, however, and these two other birds, sing also in the day, but are not then distinguished in the general concert.

The next point of superiority in a nightingale is its continuance of song, without a pause, which I have observed sometimes not to be less than twenty seconds. Whenever respiration, however, became necessary, it was taken with as much judgment as by an opera singer.

The skylark again, in this particular, is only second to the nightingale *.

* I shall here insert a table, by which the comparative merit of the British singing birds may be examined, the idea of which I have borrowed from Mons. de Piles, in his *Cours de Peinture par Principes*. I shall not be surprized, however, if, as he suggests, many may disagree with me about particular birds, as he supposes they will do with him, concerning the merits of painters.

As I have five columns instead of the four which M. de Piles uses, I make 20 the point of absolute perfection, instead of 16, which is his standard.

	Mellow- ness of tone.	Sprightly notes.	Plaintive notes.	Compass.	Execu- tion.
Nightingale	19	14	19	19	19
Skylark	4	19	4	18	18
Woodlark	18	4	17	12	8
Titlark	12	12	12	12	12
Linnet	12	16	12	16	18
Goldfinch	4	19	4	12	12
Chaffinch	4	12	4	8	8
Greenfinch	4	4	4	4	6
Hedge-sparrow	6	0	6	4	4
Aberdavine (or Siskin) . .	2	4	0	4	4
Redpoll	0	4	0	4	4
Thrush	4	4	4	4	4
Blackbird	4	4	0	2	2
Robin	6	16	12	12	12
Wren	0	12	0	4	4
Reed-sparrow	0	4	0	2	2
Black-cap, or the Norfolk Mock nightingale *	14	12	12	14	14

* Brit. Zool. p. 262.

And here I must again repeat, that what I describe is from a caged nightingale, because those which we hear in the spring are so rank, that they seldom sing any thing but short and loud jerks, which consequently cannot be compared to the notes of a caged bird, as the instrument is overstrained.

I must also here observe, that my Nightingale was a very capital bird; for some of them are so vastly inferior, that the bird-fanciers will not keep them, branding them with the name of Frenchmen*.

But it is not only in tone and variety that the Nightingale excells; the bird also sings (if I may so express myself) with superior judgement and taste.

I have therefore commonly observed, that my Nightingale began softly like the ancient orators;

I have made no mention of the bulfinch in this table, which is commonly considered as a singing bird; because its wild note, without instruction, is a most jarring and disagreeable noise.

I have likewise omitted¹ the redstart (which is called by the French *le Rossignol de Muraille*), as I am not sufficiently acquainted with its song, though it is admitted by many; I should rather conceive, however, with Zinanni, that there is no very extraordinary merit in the notes.

The London bird-catchers also sell sometimes the yellow hammer, twite and brambling^m as singing birds; but none of these will come within my definition of what may be deemed so.

* One should suppose from this, that the nightingale-catchers had heard much of the French music; which is possibly the case, as some of them live in Spittal-fields.

¹ Il culo ranzo é un ucello, (per quanto dicono) molto canoro, ma io tale non lo stimo. Delle uova é del nidi, p. 53.

^m They call this bird a kate.

reserving its breath to swell certain notes, which by this means had a most astonishing effect, and which eludes all verbal description.

I have indeed taken down certain passages which may be reduced to our musical intervals; but though by these means one may form an idea of some of the notes used, yet it is impossible to give their comparative durations in point of musical tune, upon which the whole effect must depend.

I once procured a very capital player on the flute to execute the notes which Kircher hath engraved in his *Musurgia*, as being used by the nightingale; when, from want of not being able to settle their comparative duration, it was impossible to observe any traces almost of the nightingale's song.

It may not be improper here to consider, whether the nightingale may not have a very formidable competitor in the American mocking-bird*; though almost all travellers agree, that the concert in the European woods is superior to that of the other parts of the globe†.

As birds are now annually imported in great numbers from Asia, Africa, and America, I have frequently attended to their notes, both singly and in concert, which are certainly not to be compared to those of Europe.

Thomson, the poet, (whose observations in natural history are much to be depended upon) makes

* *Turdus Americanus minor canorus*. Ray's Syn. It is called by the Indians *Contlatolli*; which is said to signify four hundred tongues. See also Catesby.

† See Rochefort's *Hist. des Antilles*, T. I. p. 366.—Ph. Fr. Abr. Vo. III. p. 563.—and Catesby.

this superiority in the European birds to be a sort of compensation for their great inferiority in point of gaudy plumage. Our goldfinch, however, joins to a very brilliant and pleasing song, a most beautiful variety of colours in its feathers*.

It must be admitted, that foreign birds, when brought to Europe, are often heard to a great disadvantage; as many of them, from their great tameness, have certainly been brought up by hand, the consequence of which I have already stated from several experiments. The soft-billed birds also cannot be well brought over, as the *succedaneum* for insects (their common food) is fresh meat, and particularly the hearts of animals.

I have happened, however, to hear the American mocking-bird in great perfection at Mess. Vogle's and Scott's, in Love-lane, Eastcheap.

This bird is believed to be still living, and hath been in England these six years. During the space of a minute, he imitated the woodlark, chaffinch, blackbird, thrush, and sparrow. I was told also, that he would bark like a dog; so that the bird seems to have no choice in his imitations, though his pipe comes nearest to our nightingale of any bird I have yet met with.

With regard to the original notes, however, of this bird, we are still at a loss; as this can only be

* I cannot but think, that there would be a demand for these birds in China, as the inhabitants are very sedentary, and bird cages are commonly represented as hanging in their rooms. I have been informed, by a Tyroleze, that his best market for Canary birds was Constantinople.

known by those who are accurately acquainted with the song of the other American birds.

Kalm indeed informs us, that the natural song is excellent*; but this traveller seems not to have been long enough in America to have distinguished what were the genuine notes: with us, mimics do not often succeed but in imitations.

I have little doubt, however, but that this bird would be fully equal to the song of the Nightingale in its whole compass; but then, from the attention which the *mocker* pays to any other sort of disagreeable noises, these capital notes would be always debased by a bad mixture.

We have one mocking bird in England, which is the skylark; as, contrary to a general observation I have before made, this bird will catch the note of any other which hangs near it; even after the skylark note is *fixed*. For this reason, the bird-fanciers often place the skylark next one which hath not been long caught, in order, as they term it, to keep the caged skylark *honest*.

The question, indeed, may be asked, why the wild skylark, with these powers of imitation, ever adheres to the parental note; but it must be recollected, that a bird when at liberty is for ever shifting its place, and consequently does not hear the same notes eternally repeated, as when it hangs in a cage near another. In a wild state therefore the skylark adheres to the parental notes; as the parent cock attends the young ones, and is heard by them for a considerable time.

I am aware also, that it may be asked, how birds originally came by the notes which are peculiar to each species. My answer, however, to this is, that the origin of the notes of birds, together with its gradual progress, is as difficult to be traced, as that of the different languages in nations.

The loss of the parent-cock at the critical time for instruction hath undoubtedly produced those varieties, which I have before observed are in the song of each species; because then the nestling hath either attended to the song of some other birds; or perhaps invented some new notes of its own, which are afterwards perpetuated from generation to generation, till similar accidents produce other alterations. The organs of some birds also are probably so defective, that they cannot imitate properly the parental note, as some men can never articulate as they should do. Such defects in the parent bird must again occasion varieties, because these defects will be continued to their descendants, who (as I before have proved) will only attend to the parental song. Some of these descendants also may have imperfect organs; which will again multiply varieties in the song.

The truth is, as I before observed, that scarcely any two birds of the same species have exactly the same notes, if they are accurately attended to, though there is a general resemblance.

Thus most people see no difference between one sheep and another, when a large flock is before them. The shepherd, however, knows each of them, and can swear to them, if they are lost; as can the Lincolnshire goatherd to each goose.

As I now draw towards a conclusion of both my experiments and observations on the singing of birds; it may be possibly asked, what use results either from the trouble or expence which they have cost me; both of which I admit to have been considerable.

I will readily own, that no very important advantages can be derived from them; and yet I shall not decline suggesting what little profit they may possibly be of, though at best they should rather be considered as what Lord Bacon terms, *experiments of light, than of fruit.*

In the first place, there is no better method of investigating the human faculties, than by a comparison with those of animals; provided we make it without a most ungrateful wish of lowering ourselves, in that distinguished situation in which we are placed.

Thus we are referred to the ant for an example of industry and foresight, because it provides a magazine of food for the winter, when this animal is in a state of torpidity during that season; nor are we less willing to suppose the song of birds to be superior to our own musical powers.

The notes of many birds are certainly very pleasing, but can by no means stand in competition either with the human voice or our worst musical instruments; not only from want of the striking effects of harmony in many excellent compositions; but because, even when compared to our simple melody, expression is wanting*, without which music is so languid and inanimate.

* The nightingale, indeed, is perhaps an exception to this general observation.

But to return to the uses (such as they are) which may arise from attending to the song of birds, or from the experiments which I have given an account of.

The first of these is too much neglected by the naturalist; for, if the bird is not caught, the only means often by which either the sex or the species can be determined is the song. For example, if Monsr. Adanton had informed us whether the European swallows, which he conceived were to be seen during the winter at Senegal, had the same notes with those of Europe, it would have been going one step further in proof of the facts which he and others so much rely upon.

These experiments, however, may be said to be useful to all those who happen to be pleased with singing birds; because it is clear, that, by educating a bird under several sorts, we may often make such a mixture, as to improve the notes which they would have learned in a wild state.

It results also from the experiment of the linnet being educated under the Vengolina, that we may introduce the notes of Asia, Africa, and America, into our own woods; because, if that linnet had been set at liberty*, the nestlings of the next season would have adhered to the Vengolina song, who would again transmit it to their descendants.

* I know well, that it is commonly supposed, if you set a caged bird at liberty, it will neither be able to feed itself, nor otherwise live long, on account of its being persecuted by the wild ones. There is no foundation, however, for this notion; and I take it to arise from an excuse for continuing to keep these birds in confinement.

But

But we may not only improve the notes of birds by a happy mixture, or introduce those which were never before heard in Great-Britain ; as we may also improve the instrument with which the passages are executed.

If, for example, any bird-fancier is particularly fond of what is called the song of the Canary bird, which however must be admitted to be inferior in tone to the linner, it would answer well to any such person, if a nestling linner was brought up under a Canary bird, because the notes would be the same, but the instrument which executes them would be improved.

We learn also, from these experiments, that nothing is to be expected from a nestling brought up by hand, if he does not receive the proper instruction from the parent cock : much trouble and some cost is therefore thrown away by many persons in endeavouring to rear nestling nightingales, which, when they are brought up and fed at a very considerable expence, have no song which is worth attending to.

If a woodlark, or skylark, was educated, however, under a nightingale, it follows that this charge (which amounts to a shilling per week *) might be in a great measure saved, as well as the trouble of chopping fresh meat every day.

A nightingale, again, when kept in a cage, does not live often more than a year or two ; nor does he sing more than three or four months ; whereas the

* Olina speaks of a paste which is used in Italy for nightingales ; but I cannot find that it ever answers with us ; perhaps, they bring their nightingales up by hand, and so accustom them from their earliest infancy to such food.

scholar pitched upon may not only be more vivacious, but will continue in song nine months out of the twelve.

I fear, however, that I have already dwelt too long upon these very minute and trifling advantages which may result from my experiments and observations; I shall therefore no longer defer subscribing myself,

Dear Sir,

Your most faithful,

Humble Servant,

Daines Barrington:

Received January 8, 1773.

XXXII. *An Account of the Tokay and other Wines of Hungary, by Sylvester Douglass, Esquire: Communicated by Edward Poore, Esq, F. R. S.*

Read June 10, 1773. **A**S the growth, quantity, and value of the Tokay wine are little known in this country, and the popular notions concerning them are in general erroneous, it may be a matter both of some curiosity and use, to throw together what I was able to collect on this subject, in the country where it grows, as well as from the chief proprietors of it, both in Hungary and at Vienna. I shall subjoin a brief account of the other most remarkable Hungarian wines.

Geography. The town, or rather village, of Tokay, from whence this celebrated wine derives its name, stands at the foot, and to the east of a high hill, close by the conflux of the river Bodrug, with the Theis or Tibiscus. In the Norimberg map of Hungary, it is erroneously placed between these rivers, for it is on the west side of both. The inhabitants are chiefly either Hungarians of the Protestant religion, or Greeks, who came originally from Turkey, but have

have been long settled here for the purpose of carrying on the wine trade. During the two last centuries, this country being almost the constant theatre of war, there was a citadel near the town, but now there is not the smallest vestige of it remaining.

The hills on which the wine grows lie all to the west of the river Bodrog and beginning close by the town of Tokay, extend westward and northward from thence, and occupy a space of perhaps ten English miles square; but they are interrupted and interspersed with a great many extensive plains, and several villages, such as Talia, Madá, Tarczal, Szombor, Benye, and Tolefwa. Near some of these, particularly Talia and Tarczal, the wine is better than what grows on the hill of Tokay, but it all goes under the same general name.

The vineyards extend beyond the 48th degree of Latitude. northern latitude.

The soil, on all the hills where the wine grows, Soil. is a yellow clayish earth, extremely deep, and there are interspersed through it large loose stones, which, as I was told, are limestone; but I had not an opportunity of examining them.

As the hills do not run in a regular chain, but are Exposure. scattered among the intervening plains, you meet with all kinds of exposures upon them, and there is wine on them all, except perhaps where they are turned directly towards the south. Yet the general rule is, that the exposures most inclining to the south, the steepest declivities, and the highest part of those declivities, produce the best wine. These circumstances shew the advantage of choosing your wine on the ground. The late Mr. Wortley Mon-

tague was so well convinced of this, that a few years before his death he undertook a journey from England to Tokay, and continued there several months, in order to be sure of having the best and most genuine wine.

Quantity. It is a vulgar error, that the Tokay wine is in so small quantity, as never to be found genuine, unless when given in presents by the court of Vienna. The extent of ground on which it grows is a sufficient proof to the contrary. It is a common defect wine in all the great families at Vienna, and in Hungary, and is very generally drank in Poland and Russia, being used at table in those countries, like Madeira in this.

Proprietors. Another vulgar error is, that all the Tokay wine is the property of the Empress Queen. She is not even the most considerable proprietor, nor of the best wine; so that every year she sells off her own, and purchases from the other proprietors, to supply her own table, and the presents she makes of it. The greatest proprietor is the Prince Trautzon, an old man, at whose death, indeed, his estate will escheat to the crown; but many others of the German and Hungarian nobility have large vineyards at Tokay; most of the gentlemen in the neighbourhood have part of their estates there; the Jesuits college at Ungwar has a considerable share of the best wine; and besides these, there are many of the peasants who have vineyards, which they hold of the Queen, or other lords, by paying a tythe of the annual produce.

Grapes. There is never any red wine made at Tokay, and, as far as I recollect, the grapes are all white.
They

They are supposed to have a particular flavour, which I own I could not perceive, though they were beginning to be ripe when I was there, in the end of August (1768), and I have often eat of such as have been brought to Vienna.

The vintage is always as late as possible. It com-^{Vintage.} monly begins at the feast of St. Simon and Jude, October 28, sometimes as late as St. Martin's, November 11. This is determined by the season, for they have the grapes on the vines as long as the weather permits, as the frosts, which from the end of August are very keen during the nights, are thought to be of great service to the wine. By this means it happens, that when the vintage begins, a great many of the grapes are shrivelled, and have, in some measure, the appearance of dried raisins.

There are four sorts of wine made from the same^{Different} grapes, which they distinguish at Tokay by the^{sorts.} names of Essence, Auspruch, Maßlsch, and the common wine.

The process for making them is as follows.

The half-dried and shriveled grapes, being care-^{1. The Es-} fully picked out from the others, are put into a per-^{sence.} forated vessel, where they remain as long as any juice runs off by the mere pressure of their own weight. This is put into small casks, and is called the Es-
sence.

On the grapes from which the essence has run off,^{2. Auspruch.} is poured the expressed juice of the others from which they had been picked, and then they tread them with their feet. The liquor obtained in this manner stands to ferment during a day or two, after which it is poured into small casks, which are kept
in .

in the air for about a month, and afterwards put into the cellars. This is the *Aufspruch*.

3. *Maislsasch*. The same process is again repeated, by the addition of more of the common juice to the grapes which have already undergone the two former pressures, only they are now also wrung with the hands, and this gives the *Maislsasch* *.

The fourth kind. The fourth kind is made by taking all the grapes together at first, and submitting them to the greatest pressure. It is chiefly prepared by the peasants, who have not a sufficient quantity of grapes, and cannot afford the time and apparatus necessary for making the different sorts. It is, I believe, entirely consumed in the country, and forms the common *Vin du pais*.

The Effence is thick, and never perfectly clear, very sweet and luscious. It is chiefly used to mix with the other kinds, and when joined to the *Maislsasch*, forms a wine equally good with the *Aufspruch*, and often sold for it.

The *Aufspruch* is the wine commonly exported, and what is known in foreign countries under the name of Tokay. The following are the best rules for judging of it; though in this, and all similar cases, it requires experience to be able to put such rules in practice.

1. The colour should neither be reddish (which it often is) nor very pale, but a light silver.

2. In trying it, you should not swallow it immediately, but only wet your palate and the tip of the

* I cannot answer for the orthography of this and the foregoing term, having written them by the ear, as they were pronounced.

tongue. If it discover any acrimony to the tongue (or bite it), it is not good. The taste ought to be soft and mild.

3. It should, when poured out, form globules in the gla'ss, and have an oily appearance.

4. When genuine, the strongest is always of the best quality.

5. When swallowed, it should have an earthy astringent taste in the mouth, which they call the *Taste of the root*. The Poles, particularly, are fond of this astringency and austerity in their Tokay. There is so great a difference between the Tokay used in Poland and what I drank both at Tokay and Vienna, which, I am sure, was of the best and most genuine kind, that I am inclined to think their wine is composed of the Maslsch, which, by the severe pressure it suffers, must carry with it much of the astringent quality which, in all grapes, resides in the skin, and a smaller proportion than usual of the essence. But this is mere conjecture.

Besides the qualities already mentioned, all Tokay wine has an aromatic taste; so peculiar, that nobody, who has ever drank it genuine, can confound it with any other species of wine. The only species that bears a resemblance to it grows, in a very small quantity, in the Venetian Friule, and is only to be met with in private families at Venice, where, in the dialect of the place, it is called *Vin piccolit*.

The Tokay wine, both the Essence and Auspruch, Age- keeps to any age, and improves by time. I have drank of the latter at Vienna, which had been in the same cellar since the year 1686. It is never good till it is about three years old.

Measure.

All the sorts are generally kept in small casks, called antheils, which *legally* hold 80 Hungarian mediæ, a measure containing about two thirds of an English quart. When you buy it of the gentlemen who are proprietors, you have commonly more than the legal quantity in the antheil; if from the Greek merchants, always less.

Price.

The particular year, or vintage, and the age, vary the price of this as of all other wines.

The medium price of the antheil of Essence is between 60 and 70 ducats. It is sometimes sold on the spot for more than 100. Prince Radzivil paid 300 ducats for two antheils about four years ago. When the price is 60 ducats, and the antheil large measure, that is, about 90 mediæ, it is exactly a ducat the English quart.

The price of the Auspruch is from 26 to about 30 ducats the antheil. This is at the rate of two florins, or near a crown the English quart. The Empress Queen, a little before I was at Tokay, purchased some of vintage 1754 at 33 ducats, and this was looked upon as an extraordinary price. The variety in the prices of the Essence and Auspruch, accounts for the opposite relations of people, who say sometimes that it costs half a guinea, sometimes five shillings, on the spot.

There are people who come every year from Poland, about the time of the vintage, to choose their own wine on the ground, and see it carefully managed. But it is a false opinion of many, that they contract for the wine of several years forwards: no such thing has ever been practised. For these last twenty years the court of Petersburg has had an

agent, who resides constantly at Tokay, for the purpose of buying wine. The present agent is a major in the Russian service, and formerly was a major-general. He commonly purchases every year from 40 to 60 antheils of Auspruch, but never of any other sort.

One of the principal proprietors, who resides at Caschaw*, told me, that he would engage to deliver the best old Tokay (i. e. Auspruch), in bottles, at London, for a ducat a bottle, containing two thirds of a quart, or considerably cheaper in cask, and would put his own seal on it, and insure it.

It is much the best way to transport it in casks; for when it is on the seas, it ferments three times every season, and refines itself by these repeated fermentations. When in bottles, there must be an empty space left between the wine and the cork, otherwise it would burst the bottle. They put a little oil on the surface, and tie a piece of bladder on the cork. The bottles are always laid on their sides in sand.

I am persuaded an English merchant, or company of merchants, would find their account in establishing a correspondence with one of the principal proprietors in the country, or in sending an agent to reside at Tokay, who might watch the opportunity of the good vintages, choose the best exposures, and bargain with the proprietors themselves. They should have cellars there to keep the wine to a proper age, and an agent at Warsaw, and another at Dantzic, to receive it. This is the road it must take. The carriage, as I was informed, from Tokay to the fron-

* The Baron Vesey, president of the chamber of Caschaw.

tiers of Poland, costs the Polish merchants two ducats the antheil, and three florins of duty. The duties in Poland amount to about 14 florins the antheil. The carriage down the Vistula cannot be very expensive. Every charge included, I am persuaded the best old Tokay might be sold in London at half a guinea the quart bottle, which is 100 *per cent.* less than what the worst often costs.

OF THE OTHER HUNGARIAN WINES.

There is not, I believe, in Europe any country which produces a greater variety of wines than Hungary. I have been told, at Vienna, of an entertainment given to the Empress Queen at Presburg, by an Hungarian nobleman, where there were above a hundred different sorts of wine produced, all the growth of that country. Many of them are of an excellent quality, and would undoubtedly be a source of great commerce and riches to that kingdom, if it enjoyed a maritime, instead of its inland, situation.

The most valuable white wines after the Tokay, are,

1. *The St. George wine*, which grows near a village of that name, about two German miles north of Presburg, and in the same latitude with Vienna. This wine approaches the nearest of any Hungarian wine to Tokay. Formerly they used to make *Auspruck* at St. George; but this was prohibited by the court about sixteen years ago, it being supposed that it might hurt the traffic of the Tokay wine. The prohibition shews, that the method of preparing
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the Tokay is thought to have a share in its peculiar qualities.

2. *The Edenburg wine*, resembling the St. George, but inferior in quality and value. Edenburg is a town situate about nine German miles north-west of Presburg.

3. *The Carlowitz wine*, something like that of the Côte rotie on the banks of the Rhone. Carlowitz is the seat of the metropolitan of the Greek church in Hungary. It stands on the banks of the Danube, between 45 and 46 degrees of latitude.

The best red wines are,

1. *The Buda wine*, which grows in the neighbourhood of the antient capital of the kingdom. This wine is like, and perhaps equal to, Burgundy, and is often sold for it in Germany. A German * author of the last century says, that a great quantity of this wine used to be sent to England in the reign of James I. over land by Breslaw and Hamburg, and that it was the favourite wine both at court and all over England.

2. *The Sexard wine*, a strong deep-coloured wine, not unlike the strong wine of Languedoc, which is said to be sold at Bordeaux for claret. The Sexard wine on the spot costs about 5 creuzers, or $2\frac{1}{2}$ d. a bottle. It belongs to the Abbot of Constance, and is chiefly consumed in Germany. Sexard is on the Danube, between Buda and Essek.

3. *The Erlaw wine*, which is reckoned at Vienna almost equal to that of Buda. Erlaw is in Upper

* Hevelius Silesiograph. 2 vols. art. *Breslaw*.

Hungary, south-west of Tokay, between 47 and 48 degrees of latitude.

4. *The Gros Wardein wine*, a strong bodied wine, and very cheap. It belongs chiefly to the Duke of Modena, whose ancestor got a large estate in this country, in grant from the Emperor Leopold, as a reward for his services in the Hungarian wars. Gros Wardein is an old fortress near the confines of Transylvania, between 46 and 47 degrees of latitude.

XXXIII. *On the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules. By Mr. William Hewson, F. R. S. and Teacher of Anatomy.*

Read June 17, and 24, 1773. **T**HE red particles of the blood in the human subject have, since the time of Leeuwenhoeck, been so generally allowed to be spherical, that in almost all books of physiology they are denominated red globules. A few authors, however, have at different times doubted whether they were spheres, and amongst the rest Father de la Torr , whose curious observations, together with his glasses, were presented to the Royal Society, Anno 1766. As I flatter myself that I have made some new observations on these particles, I shall do myself the honour of communicating them to the Society.

It is a curious and important fact, that these particles are found so generally through the animal kingdom; that is, they are found in the human species, in all quadrupeds, in all birds, in all amphibious animals, and in all fish, in which animals they are red, and colour the blood. The blood even of insects contains particles similar in shape to those of the blood of more perfect animals, but differing

fering in colour. In water insects, as lobsters and shrimps, these particles are white; in some land insects, as the caterpillar and the grass-hopper they appear of a faint green, when in the vessels as I am persuaded from experiments. I have seen them in an insect no bigger than a pin's head, and suspect they exist almost universally through the animal kingdom.

What is so generally extended through the creation must be of great importance in animal œconomy, and highly deserving the attention of every enquirer into the works of nature. This subject becomes the more interesting from so much of reasoning in the theory of medicine being built in the properties of those particles.

It is by the microscope alone that we can discover these particles; and as some dexterity and practice is required in the use of that instrument, there have not been wanting men of character and ingenuity, who, having been unsuccessful in their own experiments, have questioned the validity of those made more fortunately by others. Some have gone so far as to assert, that no credit can be given to microscopes, that they deceive us by representing objects different from what they really are. These assertions, though not entirely without foundation, when we speak of one sort of microscopes, are very unjustly applied to them all. In compound microscopes, when the object is viewed through two or more glasses, if these glasses be not well adapted to the focus of each other, the figure of the object may be distorted; but no such circumstance takes place, when we view an object through a single lens. All who use spectacles agree, that the figures of objects appear the same through them, as they
do

do to the naked eye. And as the single microscope has, like the spectacle, but one lens between the eye and the object, there is no reason to suppose the one can deceive us more than the other. The compound, having a larger field, is more pleasant than the single microscope for many purposes; but the single should be always preferred by those who wish to ascertain the figures of minute bodies. It was this instrument supported on a scroll, as delineated by Mr. Baker (*Microscope made easy*, plate II. chap. 3.) that has been used in these experiments: and almost all the observations were made with lenses, as they are prepared by some of our more skilful workmen in London. One observation only was made by means of those globules made of glass, which the ingenious Father de la Torr  presented to the Royal Society, and which they were so obliging as to lend to me. Of these globules but two were fit for use, when they came into my hands, viz. that which, according to Father de la Torr , magnifies the diameter of the object 640 times, and that which magnifies 1280 times. The lenses of the greatest magnifying power made in London are those of $\frac{1}{4}$ th of an inch focus, which, even allowing eight inches to be the focal distance of the naked eye, magnifies the diameter of the object only 400 times; a power much inferior to what may be obtained by globules, and particularly by that globule, which according to Father de la Torr  magnifies the diameter 1280 times; and this globule I have used in some of these experiments. But our lenses, though inferior in magnifying power to these globules, are much superior in distinctness; the
globules

globules are full of clouds, made by the smoke of the lamp used in preparing them, and the object can be seen only through the transparent parts of the globules, which makes it difficult to get a satisfactory view of it; this, with the trouble of adapting the objects to the focus of the glass, made me prefer our own lenses for all the experiments mentioned in these sheets, except one; and it is but doing justice to the ingenious gentleman abovementioned, to acknowledge, that the greater power of his glasses was found in that experiment more than to compensate for their want of distinctness.

These particles of the blood, improperly called globules, are in reality flat bodies. Leeuwenhoeck and others have allowed, that in fish and in the *amphibia* they are flat and elliptical; but in the human subject and in quadrupeds almost all microscopical observers have agreed in their being spherical. When we consider how many ingenious persons have been employed in examining the blood with the best microscopes, it will appear wonderful, that the figure of these particles should have been mistaken; but our wonder will be lessened, when we consider how many obvious things are overlooked, till our attention is very particularly directed to them; and besides, the blood in the human subject and in quadrupeds is so full of these particles, that it is with great difficulty we can see them separate, unless we find out a method of diluting the blood. It is to such a discovery that I attribute my success in this enquiry; for, having examined the blood as it flows from the vessels of the human body, it appeared a confused mass, notwithstanding
I spread

I spread it thin on a glass, or a piece of talk. It then occurred to me to dilute it, but not with water, for this I knew dissolved the particles; but with serum, in which they remain undissolved. By the serum I could dilute it to any degree, and therefore could view the particles distinct from each other; and in these experiments I found that these particles of the blood were as flat as a guinea. I likewise observed that they had a dark spot in the middle, which Father de la Torr  took for a hole; but, upon a careful examination, I found it was not a perforation, and therefore that they were not annular. I next made experiments by mixing these particles with a variety of other fluids, and examined them in many different animals; and the result of these experiments was, that their size is different in different animals, as is seen in plate XII, where they are represented of the size they appeared to my eye, when viewed through a lens of $\frac{1}{4}$ of an inch focus, which, allowing eight inches to be the focal distance of the naked eye, magnifies the diameter 184 times.

It may not be improper to observe here, that the accurate L cuwenhoeck not having diluted the human blood, or that of quadrupeds, so as to see these particles separate from each other, was thence not qualified to describe them from his own observation, as he has done those of fish and of frogs; and, suspecting a round figure was more fit for circulating in our vessels, was thence led to suppose these particles spherical in the human subject. But I shall hereafter be able to shew from his own words, that it is not his observations, but

his speculative opinions, or his theory, that differs from what I have discovered by these experiments.

In plate XII. it appears, that of all the animals which I have examined, the particles are largest in the fish called a skate; next to the skate they are largest in a frog and a viper, and other animals of this class; they are somewhat smaller in the common fish, as the salmon, cod, and eel. In birds they are smaller than in fish; in the human subject smaller than in birds; and in some quadrupeds still smaller than in the human subject. Leeuwenhoeck, speaking of their size, says, he is confident the red particles of the blood are no larger in a whale than in the smallest fish *. And others have, since his time, said they are of the same size in all animals; but it is evident, from comparing their size, as delineated in the abovementioned plate, that it differs considerably, and that they are not largest in the largest animals; for we find that in an ox they are not so large as in a man; and so far are they from being larger in the whale than in the small fish, it appears probable, from comparing their size, as delineated plate XII. N° II. from a porpus, which belongs to the same genus as the whale, that they are smaller in those animals than in fish. Neither is their size inversely, as the size of the animal; for they are as large in an ox as in a mouse. The difference in their size therefore depends on some other circumstance than a difference in the size of the animal.

As to their shape, I have already mentioned that they are flat in all animals, even in the human subject; of which any one may be convinced by repeating the following experiments.

EXPERIMENT I. Take a small quantity of the serum of human blood, and shake a piece of the crassamentum in it, till it is coloured a little with the red particles; then with a soft hair pencil spread a little of it on a piece of thin glass, and place this glass in the microscope in such a manner as not to be quite horizontal, but higher at one end than the other, by which means the serum will flow from the higher extremity to the lower, and as it flows, some of the particles will be found to swim on their flat sides, and will appear to have a dark spot in their middle; others will turn over from one side to the other as they roll down the glass. No person who sees them turn over can doubt of their being flat; he will see them in turning have all the phases that a flat body has; first he will see them on one side, then rise gradually upon their edge, and turn over to the other side. I have in this way shewed their figure to a number of curious persons, and particularly to many students of anatomy, who have attended lectures in London within the last six years.

If, instead of serum, the particles should be diluted with water containing rather more salt than serum does; or if, instead of human blood, that of an animal with larger particles be used: then they will sometimes be seen not only flat, but a little bended, like a crooked piece of money.

These experiments not only prove that the particles of the blood are flat, and not globular; but likewise, by proving that they are flat, they shew that they are not fluid, as they are commonly believed to be; but, on the contrary, are solid; because every fluid swimming in another, which is in larger quantity, if it be not soluble in that other fluid, becomes globular; this is the case where a small quantity of oil is mixed with a larger quantity of water, or if a small quantity of water be mixed with a large one of oil, then the water appears globular. And as these particles are not globular, but flat, they must be solid; a circumstance that will appear still more evident from future experiments.

It is necessary to remark, that in a few minutes after the particles are spread out on a glass, they run in clusters, and stick to each other, and then they appear confused.

When one of these particles is attentively examined, while separate from the rest, it appears, as it lies on its flat side, to have a dark spot in the middle, and round that dark spot it is more transparent. This dark spot was believed to be a perforation, or the particle was supposed to be a hollow ring, by the ingenious Father de la Torr . But I find, from a great number of experiments, that the dark spot is a solid particle contained in a flat vesicle, whose middle only it fills, and whose edges are hollow, and either empty, or filled with a subtle fluid. This will be evident to every one who will carefully make the following experiments.

EXPERIMENT II. Take a drop of the blood of an animal that has large particles, as a frog, a fish, or, what is still better, of a toad; put this blood on a thin piece of glass, as used in the former experiment, and add to it some water, first one drop, then a second, and a third, and so on, gradually increasing the quantity; and in proportion as water is added, the figure of the particles will be changed from a flat to a spherical shape. When much water is added, the vesicle will by degrees become thinner, and more transparent, and will at last be dissolved. When the vesicle has thus assumed a spherical shape, it will roll down the glass stage smoothly, without those phases which it had when turning over whilst it was flat; and as it now rolls in its spherical shape, the solid middle particles can be distinctly seen to fall from side to side in the hollow vesicle like a pea in a bladder. Sometimes, indeed, instead of falling from side to side, the solid middle particle is seen to stick to one part of the vesicle; and in proportion as the vesicle, instead of being flat, assumes a spherical shape, its longest diameter is shortened as might be expected, on the supposition of its being hollow and flat.

After this experiment has been made on the blood of such animals as have large vesicles, it may be made on human blood, where the water will be found to have the same effect; the vesicles will become spherical, the diameters of these spheres will be less than the largest diameter of the vesicle was, in its flat state.

It is remarkable that more water is in general required to produce this change on the vesicles of the human blood than on those of frogs, or other amphibious animals; and those of the amphibia require still more than those of fish; for the substance of these vesicles being thicker and more coloured in man and in quadrupeds than in the amphibia, is therefore later in being dissolved in water; and being thinnest in fish, it thence most readily dissolves. Those who are desirous of repeating these experiments had best begin with the blood of toads and frogs, whose vesicles are large, and remain some time without dissolving in the water (when that is used with the above-mentioned precautions); by which means any one accustomed to microscopical experiments may readily be satisfied of these curious circumstances.

From the greater thickness of the vesicles in the human subject, and from their being less transparent when made spherical by the addition of water, and likewise from their being so much smaller than those of fish or frogs, it is more difficult to get a sight of the middle particles rolling from side to side in the vesicle, which has become round; but with a strong light *, and a deep magnifier, I have distinctly seen it in the human subject, as well as in the frog, toad, or skate.

Since water makes these particles round, and makes the dark spot in their middle disappear, it is evident the red particles of the human blood are not perforated; but that dark spot is owing to something else than a hollow and this is likewise con-

* These experiments were all made with day-light, in clear weather.

firmed by observing that although the particle does in an obscure glass appear only to have a dark spot which might be supposed to be a hole, yet, with a very transparent lens, and a good light, after diluting the blood with serum, that middle part can be distinctly seen to be only of a deeper red than the rest of the vesicle, and thence appears darker.

In these experiments, made by adding water to the blood, the middle particles appear to be less easily soluble in water than the flat vesicle which contains them; so that, a little time after the proper quantity of water has been added, the flat vesicles disappear, leaving their middle particles; which seem to be globular, and very small.

That these red vesicles of the blood, although flat, are not perforated, is evident likewise from a curious appearance which I have repeatedly observed in blood that has been kept three days in the summer season, so that it was beginning to putrefy; the vesicles of this blood being diluted with serum, and examined with a lens $\frac{1}{8}$ of an inch focus, but more particularly when examined with M. de la Torr 's glass, which, by his computation, magnifies the diameter 1280 times, were found to have become spherical; the diameter of these spheres was less than their largest diameter when flat, and their external surface was corrugated in such a manner as to make them appear like small mulberries.

I have seen the same appearance on mixing serum that had been kept three days in a warm place, and smelt putrid, with fresh-drawn human blood; the vesicle assumed this globular and mulberry-like appearance.

In these experiments on human blood beginning to putrefy, I have likewise observed some of these vesicles break into pieces without becoming spherical; and I have distinctly perceived the black spot in the center fissured through its middle, another proof that it is not a perforation.

In the blood of an eel, which was beginning to putrefy, I have seen the vesicles split and open, and the particle in its center come out of the fissure.

As the putrefaction advances, those vesicles which had become rough spheres, or like mulberries, and those which had been merely fissured, each break down into smaller pieces. M. de la Torré seems to think they have joints, and break regularly into seven parts; and Leeuwenhoeck suspected these globules, as he called them, were constantly made of six lesser globules. But from observations I am convinced there is nothing regular or constant in the number of pieces into which they break. I have seen them fall into six, seven, eight, or more pieces, by putrefaction; for putrefaction breaks them down in the manner it destroys other animal solids.

I need hardly take notice, that the small pieces into which the vesicles break are equally red as the vesicle itself. The theory of the red globules being composed of six ferrous ones compacted together, and the ferrous globules of six of lymph, has not the least foundation, and is intirely overthrown by the simple experiment of mixing the blood with six or with thirty-six times its quantity of water; for the water dissolving the globules ought to reduce them to yellow serum, or colourless lymph *: but it does

* See Gaubii Pathologia.

not; on the contrary, it is coloured red by these particles, even when used in much greater proportion than thirty-six parts of water to one of blood.

These red vesicles of the blood have not only been commonly supposed globular and fluid, but they have with equal injustice been imagined to be oily and more inflammable than the rest of the blood. That they are not oily is evident from their so readily dissolving in water; and that they are not more inflammable than the rest of the blood is manifest by burning them after they are separated from the rest of the blood, which separation may be effected by shaking the crassamentum in the serum so as to diffuse the particles through it; and then, by pouring off the clear serum, when they have subsided in it. I have separated them in this manner, and compared their inflammability with that of inspissated serum, and of dried coagulable lymph, and have not observed them more inflammable than the serum or the lymph; nor do they melt like oils, as some have suspected, but burn like a piece of horn.

Some authors, who have written on the figure of these vesicles in quadrupeds, and in the human subject, have expatiated on the great advantages of their (supposed) spherical shape, in order for their more easy circulation; as it is probable that no form is preferable to a spherical one for easy motion. But as these vesicles are evidently not spherical but flat in all animals, we must believe that nature has some good purpose to answer by making them of that form.

It has been objected, that, notwithstanding they appear flat out of the body, they may possibly be

globular in the body, while circulating; and it has been said, that it is almost inconceivable that so many ingenious men should at different times have viewed them through a microscope, and have concluded them spherical, if they be really flat. But, however that may have happened, it is a fact that they are as flat in the body as out of it. Of this I am convinced by having repeatedly observed them whilst circulating in the small vessels between the toes of a frog, both in the solar microscope, and the more simple one abovementioned. I have seen them with their sides parallel like a number of coins laid one against another. I have likewise in that animal, where they are elliptical, seen them move with one end foremost, and sometimes with an edge turned towards the eye. I have moreover seen them, when entering a small vessel, strike upon the angle between it and the larger trunk, and turn over with the same variety of phases that they have when turning over upon a piece of glass.

Upon this occasion I may remark, that it has been said by some microscopical observers, that in passing through very small vessels they seem to alter their shapes, and to be lengthened. This conclusion, I suspect, has taken its rise from the observer having seen them with their edge turned towards his eye; in which case they would appear, long and small, as if lengthened by compression, especially to one who sets out with the notion of their being globular. I have seen them in blood vessels, which would admit only single vesicles, move with difficulty, as if streightened for room; but never saw them altered in their shape by the action of the vessels.

If then they really be not globular but flat, and if water so readily alter their shape, whence is it that the serum has the property of preserving them in that form which seems so necessary, because it is so general through the animal creation?

It is principally by the salts of the serum that this effect is produced, as is proved by adding a small quantity of any neutral salt to water, when the water is no longer capable of dissolving those particles, nor does it alter their shape when the salt is used in a certain proportion.

EXPERIMENT III. If a saturated solution of any of the common neutral salts be mixed with fresh blood, and the globules (as they have been called; but which for the future I shall call flat vesicles) be then examined in a microscope; the salt will be found to have contracted or shriveled the vesicles; so that they appear quite solid; the vesicular substance being closely applied all round the central piece. In proportion as the solution of salt is diluted with water, it has less effect; and, when diluted with six, eight, ten, or twelve times its quantity of water, it produces no change in the figure of the vesicles, whose flat shape can then be seen even more distinctly than when mixed with serum itself.

The neutral salts, which, when diluted with water, have been observed to have the effects above-described, are Glauber's salt, Epsom salt, a salt formed of the volatile alkali and the vitriolic acid, common nitre, cubic nitre, a salt made with the volatile alkali and the nitrous acid, as well as the salts made with the nitrous acid and magnesia, or

with the nitrous acid and chalk, and also common salt, digestive salt of Sylvius, and a salt made with vinegar and the fossil alkali. These experiments were sufficient to convince me, that this property was very general among those salts which consist of acid and alkali; and therefore it seemed unnecessary to prosecute this enquiry farther *.

But acids and alkalies have different effects on these vesicles from what neutral salts have.

The fixed vegetable alkali, and the volatile alkali, were tried in a pretty strong solution, and found to corrugate the vesicles; and in proportion as they were diluted, their effects became similar to water alone, but it is not easy to find the point of strength where the vesicles would remain unaltered in the solution. And here we may observe, that since these vesicles are found to dissolve so readily in water, and not to be dissolved in these solutions of alkali, it is a strong argument against their being either oily or saponaceous, as they have been suspected.

The effects of acids are very different. I have tried the vitriolic, nitrous, muriatic distilled vinegar, and the acid of phosphorus; these, when much diluted, have the same effects as water in making the vesicles spherical; and, in proportion as they are less diluted, they dissolve the vesicles without making them spherical, as water does. I never

* These experiments were made by putting one drop, of the saturated solution of the salt into a tea cup, and then adding distilled water by a few drops at a time; and to this mixture the serum of the blood, highly tinged with the red vesicles, was added.

could find any point of diffuſion where the acids like the neutral ſalts produced no change on the figure of the veſicles. This experiment is the more to be attended to, as theſe veſicles have been ſuppoſed to be oily and ſaponaceous, which is improbable, ſince they diſſolve more readily in acids than in alkalis.

Salts made with earth of alum, and any of the acids, always corrugate thoſe veſicles, unleſs they be very much diluted; when their effects are ſimilar to thoſe of the water alone, that is, they make the veſicles aſſume a ſpherical ſhape. I could not diſcover any point of ſtrength in theſe ſolutions where the particles would remain in them without being changed in their ſhape.

The ſame was obſerved of ſpirit of wine: ſome of the metalline ſalts, as copperas, ſublimate, and Roman vitriol, were tried; and when much diluted, their effects were not different from thoſe of water; but in proportion as the ſolution was ſtronger, they corrugated the veſicles more and more.

Urine, when containing much of its ſalts, has effects ſimilar to the ſerum; but in proportion as it is weaker, its effects are more like thoſe of water.

The uſe therefore of thoſe ſalts which enter into the compoſition of the blood is probably to preſerve the red veſicles in their flat form; for we muſt ſuppoſe ſome advantages attend that ſhape, ſince nature has made uſe of it ſo generally in the blood of different animals. And as both a very ſtrong, ſolution of neutral ſalts and a very diluted one alters the ſhape of the veſicles, it is probable nature has
limited.

limited the proportions of the water and the salts in our blood. A degree of latitude in these proportions however seems to be admitted; for I observed the vesicles equally unchanged when mixed with a solution of salts consisting of eight drops of water to one of the saturated solution, and when added to a mixture of fifteen drops of water to one of the same solution.

Not only the neutral salts in the blood are capable of preventing the serum from dissolving the vesicles; but the mucilage or lymph with which the serum is so much impregnated, seems to contribute to the same effect.

When the vesicles have been made spherical, by being mixed with water, if a small quantity of pretty strong solution of a neutral salt be added, they are immediately shriveled, a few of them recover their former flat shape, but the greatest part are contracted irregularly into smaller spheres. When these vesicles thus recover their shape, after having been a short time mixed with water, they are generally more transparent, and appear thinner, a part of their substance having been dissolved in the water; and thence it is more easy to distinguish the little solid particle which is contained in them. By this experiment I have had the pleasure of convincing many curious persons of the composition of this part of the blood, who were not quite satisfied from some of the other experiments.

I have mentioned above, and have shewn in plate XII. that these vesicles are of different sizes in different animals. I have likewise observed, that they are not all of the same size in the same animal, some

some being a little larger than others; and some dissolve in water more readily than others. In the same species of animals they even differ in size in the different periods of life. In a chicken on the sixth day of incubation I found them larger than in a full-grown hen, as is represented in the plate; and I found them larger in the blood of a very young viper than in that of its mother, out of whose belly it was taken. I have not however been convinced from experiments, that there is any difference in size between those of a child at its birth, and those of an adult person.

In the blood of some insects the vesicles are not red, but white, as may easily be observed in a lobster (which Linnæus calls an insect); one of whose legs being cut off, a quantity of a clear *sanies* flows from it; this, after being some time exposed to the air, jellies, but less firmly than the blood of more perfect animals. When it is jellied, it is found to have several white filaments; these are principally the vesicles concreted, as I am persuaded from the following experiment.

EXPERIMENT IV. If one of the legs of a lobster be cut off, and a little of the blood be caught upon a flat glass, and instantly applied to the microscope, it is seen to contain flat vesicles that are circular, like those of the common fish, and have each of them a lesser particle in their center as those of other animals. But there is a curious change produced in their shape by being exposed to the air; for soon after they are received on the glass, they are corrugated; or from a flat shape are changed into regular spheres, as is represented in plate XII. N^o XII.

This.

This change takes place so rapidly, that it requires great expedition to apply them to the microscope soon enough to observe it.


I have observed the *sanies* or blood of a shrimp, by cutting off its tail, and found vesicles in it similar to those of the lobster, which have been a short time exposed to the air. But I never could apply the blood so as to see them in their flat form; but, since they changed by exposition to the air, I conjecture that like them they were flat in the blood vessels; but being more susceptible of changes from the contact of air, they were corrugated before I could get them applied to the microscope.


The ingenious Leeuwenhoeck has observed that in the blood of a grasshopper, its vesicles or globules, as he calls them, are green. I have seen the same circumstance in the white caterpillar, whose serum appeared green when in its vessels; but when let out from this animal or from a grasshopper, the colour cannot be distinguished.


The smallest animal in which I have discerned these vesicles is an insect no bigger than a pin's head, that is seen almost constantly in the river water which we have in London. This insect, which is a species of the *Monoculus*, being put into a concave glass with a little water, and the rays of the sun being made to pass through it, the heart may be seen to beat, and the transparent blood or *sanies* found to have a few vesicles, which appear to move one after the other; being made visible, though transparent, by the light passing in such a manner as to be refracted by them.

Since


(1) A comparative view of the **FLAT VESICLES** of the Blood in different Animals exhibiting their size & shape as they appear thro a Lens $\frac{1}{23}$ of an inch & forms

Fig I  I have seen in a Cat an Upside down & a Bat


VII  I have seen in a small Dog taken from the belly of its mother

II  II in a human blood vessel, a Dog & a Poppy


VIII  VIII in a human blood vessel

III  III in a human blood vessel, a Dog, a human blood vessel & a Duck


IX  IX in a human blood vessel


IV  IV in a human blood vessel, a Dog, a human blood vessel & a Duck


X  X in a human blood vessel

V  V in a human blood vessel, a Dog, a human blood vessel & a Duck

XI  XI in a human blood vessel

VI  VI in a human blood vessel, a Dog, a human blood vessel & a Duck

XII  XII in a human blood vessel, a Dog, a human blood vessel & a Duck

XIII  XIII in a human blood vessel, a Dog, a human blood vessel & a Duck

Since so small an animal as this has these curious vesicles equally as the larger and more perfect animals, is it not probable that they are diffused through the whole animal creation? And what is found so generally amongst animals must be of great use in their œconomy.

P. S.

Mr. Hewson begs leave to add, that since these experiments open a new field for enquiry; and as he has already so far prosecuted the subject as to be persuaded that he has thereby discovered not only the use of the lymphatic glands and of the Thymus, but also of the spleen: in order to have the opinions of the ingenious concerning the facts mentioned in this paper, he ventures to solicit, that such gentlemen as are curious in natural knowledge, and particularly the Members of the Royal Society, would honour him with their company, at his house in Craven-street, in the Strand, where he will repeat the experiments before them.

XXXIV. *Account of the Effects of a Thunder-Storm, on the 15th of March 1773, upon the House of Lord Tylney at Naples. In a Letter from the Honourable Sir William Hamilton, Knight of the Bath, his Majesty's Envoy Extraordinary at the Court of Naples, and F. R. S. to Mathew Maty, M. D. Sec. R. S.*

Naples, March 20, 1773.

S I R,

Read June 17,
1773.

ON Monday last, about half past ten at night, I had the satisfaction of being one, of many witnesses, to several curious phænomena, occasioned by the lightning having fallen on Lord Tylney's house, in this city. It was on his Lordship's assembly night; so that most of the nobility of this country, many of the foreign ministers, foreigners of distinction, particularly English, were present at the time of the explosion; to be sure there were not less than two hundred and fifty in the apartments, and, including servants, the whole number under Lord Tylney's roof could not be less than five hundred. The lightning passed through nine rooms, seven of which were crowded with

with parties at cards, or conversing; it was visible in every one, notwithstanding the quantity of candles, and was left in all, evident marks of its passage. Many of the company were sensible of a smart stroke, like that of electricity, and some complained for several days after, of a pain they felt from that stroke, but no one received any essential hurt; a servant, indeed, of the French ambassador's house has a black mark on his shoulder and thigh, from a stroke he received on the stair-case; and another servant, who was asleep on the same stair-case, his head reclining against the wall, had the hair entirely singed from it on that side.

The confusion at the moment was, as you, Sir, may well imagine, very great: the report, which seems to have been equally heard in every room, was certainly as loud as that of a pistol; and every one flying the room they were in, thinking the danger there, met of course in the door-ways, and stopped all passage. A Polish prince, who was playing at cards, hearing the report (as he thought of a pistol), and feeling himself struck, jumped up, and, clapping his hand to his sword, put himself in a posture of defence. I was sitting on a card-table, and conversing with Monsieur de Saussure, Professor of Natural History at Geneva; we happened to be looking different ways, and each of us thought that the bright light and report was immediately opposite to us: and, upon enquiry, I found that every one was persuaded that the greatest explosion had been directly before him. I thought that an Indian cracker had been fired, and Monsieur de Saussure thought it the report of a pistol; but hearing, amidst other

confused cries and noises, a voice saying, *Un fulmine, un fulmine!* we began to examine the gallery in which we were, and soon discovered that the gilding of the cornish had been affected, for in the corners, and at every junction, it was quite blackened; those that had been sitting under the cornishes were covered with the shining particles of the varnish that went over the gilding, and which was thrown off in small dust, at the moment of the explosion. There was a smell of sulphur in Lord Tylney's apartments, but not very considerable; I thought there was more in the apartment above, which Monsieur de Sauffure and I visited immediately after, and where we found the same operation had been performed on the gildings. It is very certain that the profusion of gildings, which is remarkable in this house, and the bell-wires, prevented the lightning from making more use of the company to conduct it in its course. I will endeavour to give you as clear an account as I am able, of what I saw the next morning, with Monsieur de Sauffure, when we examined together, most carefully, the whole of Lord Tylney's house; which you are at liberty to communicate to our respectable Society, if you think it worthy of its attention. There never was, I believe, an accident of the kind, that proved more clearly, the exact similitude of lightning and the electrical fluid, in all their operations.

The best apartments of this country have usually a broad cornish of lacker, or false gold, round their coved ceilings. Wood, covered with white plaister, a silvered leaf, and a yellow varnish, composes this magnificent cornish; a band of the same sort, but
much

much narrower, goes round the hangings, and down the corners, where it is double, which you will immediately comprehend, by casting your eye on the section of the gallery, marked A, of the inclosed drawing [see TAB. XIII]. The chairs, sofas, frames of pictures, tables, &c. are usually of the same sort of gilding, at least they were so here. By sending you the dimensions of each room of Lord Tylney's apartment, you will see on what a prodigious surface of gilding the lightning spread itself in its course; for you must add the same quantity of gilding in the apartments over Lord Tylney's, which are of the same dimensions, as richly decorated, and as much damaged; though we remarked that Lord Tylney's rooms that had suffered most, did not always correspond with those that suffered most in the upper apartments. Monsieur de Saussure and I began our examination on the flat roof of the house, composed of a kind of stucco, on which there was no sign of damage; neither was there on any of the chimneys. A tin gutter, with many spouts of the same metal, projecting about three feet, is immediately under this roof, and each of the spouts is supported by a small iron rod, or cramp, inserted in the wall underneath, and above by two wires of about the size of a goose-quill, and which likewise go into the wall. We observed a wire of this sort melted; and it seems highly probable, that the lightning found its way into the house at this place. We observed also, from the roof, that, though the house stands high, it is nevertheless commanded by many cupolas, and higher buildings; which, with other circumstances, makes it highly probable, that only a
 portion.

portion of the lightning, the great explosion of which was heard all over Naples, had been conducted through our assembly. In the garrets under the *attico*, or flat roof, we could perceive no signs of damage; under them, in the rich apartment immediately over Lord Tylney's, and consisting of the same number of rooms, the gilding of the cornishes, bands, chairs, sofas, &c. exhibited exactly the same appearance as in Lord Tylney's, which shall be particularly described presently. The account of the appearances at the moment of the explosion, given us by the few people that were in the apartments at the time, corresponded perfectly with what we had seen below.

Lord Tylney's apartment consists of five rooms on a line, and four others, going off at a right angle from the fourth room of that line. The lightning seems to have entered the first room of the five towards the north, and which is under that part of the gutter where we suspected it to have entered that part of the house. The five rooms of this line are of the same breadth, $23\frac{1}{2}$ feet, and the four others are $14\frac{1}{2}$ feet broad. The first room is $18\frac{1}{2}$ feet long. The gilt cornish of the whole apartment is in general $9\frac{1}{2}$ inches broad, and the gilt band that goes round the hangings $2\frac{1}{2}$ inches in breadth. The cornish of the room is only blackened at the joints, particularly the corners, and where there was any flaw or crack in the gilding. The small bands, which appear by their colours to have conducted the lightning down from the cornish in eight different parts of this room are (as in the other rooms) 14 feet

feet high, and these are what I shall call hereafter vertical descents, as marked in the drawing of the section of the room A. The gilding of a sofa in this room is likewise blackened. We found, that whenever a chair or sofa had been affected, it was owing to its having been, at the time of the explosion, in contact with the gilt band, and that the point of contact was continually marked by a black spot, both on the chair and band.

The second room, or gallery, in which Monsieur de Saussure and I were at the time the lightning fell, is $33\frac{1}{2}$ feet long; the gilding of the cornish is much damaged, particularly on that side in contact with the bell wire. The lightning in this room had ten vertical descents, and passed over the gilding of two chairs, two sofa's, and the frames of two marble tables, the white marble of which, at those parts which were in contact with the gilding, is tinged yellow, and such parts of the damask of the chairs and sofa's as were in contact with the gilding, and had nails underneath, are singed. I found, upon enquiry, that most of those who were sitting upon these particular chairs and sofa's were sensible of a smart stroke.

The third room, a section of which is marked A. in the drawing, is thirteen feet long, the cornish much damaged, a sofa and two chairs damaged; nine vertical descents in this room.

The fourth room is $14\frac{1}{2}$ feet long; the cornish is damaged, and the lightning had nine vertical descents in this room likewise, the bands being much damaged.

The fifth room is twelve feet two inches long; the cornish much hurt, particularly on the side in contact with the bell wire; there are no vertical bands in this room, therefore no apparent vertical descents of the lightning.

The sixth room, which is the angle to the fourth, is much hurt in the cornish; it is fourteen feet long, and has no gilt bands.

The seventh room nineteen feet $\frac{1}{2}$ long; no signs of damage on the gilding of the cornish, or in any other parts of the room, except the bell wire, which was melted, and seemed alone to have conducted the whole accumulated force of the lightning to the cornish of the next room.

Eighth room; a section of which is represented in the drawing under B. The cornish being overcharged, and the lightning, finding no complete vertical conductor, jumped from the picture frame over the door to the gilding of the door case, which gilding is six inches wide; and on one side where the gilding ended, it knocked out a piece of wood, which is likewise burnt, or rather singed. The track of the lightning is evidently marked on the white wall, as if by the flame of a candle, black and yellow; and the same sort of tinge is visible (as represented in the drawing) on each side of the gilding of the door. We remarked that the picture over the door was the only one in the room that was in contact, or near the cornish.

The ninth room is eighteen feet long; the cornish is hurt, and the lightning descended from it to a picture frame over the door, and from thence to another, which was the greatest jump that we remarked;
ed;

ed ; its passage is clearly worked on the wall, as is represented in the drawing C, and the distance from one picture frame to the other is thirteen inches. It went then to the other picture frame, and down to the gilding of the door case, which is surrounded in part, having made a hole in the wall, which however it did not pierce.

The lightning seems to have been much stronger in these two rooms than in any other ; and, as they were servants rooms, there were not above two or three people in them at the time of the explosion.

Underneath these apartments we found no traces of the explosion, except on the wall of the room directly under the door case of Lord Tylney's eighth room, where a piece of the plaistered wall of about six inches square was beat out, and scattered about the floor.

Under this room again was a wall in a damp wash-house, where most probably the lightning communicated with the earth, and dispersed itself.

Thus have I followed it through its course, and will take my leave of you ; but first I must tell you, that I have succeeded in discharging my battery of nine bottles over the cornices of two of my rooms, which represents in miniature exactly what we saw in such perfection at Lord Tylney's.

An excellent electrical machine that I had of Mr. Nairne is the wonder of this country ; as

they had never before seen electrical experiments in perfection.

I am,

S I R,

with great regard and esteem,

your most obedient

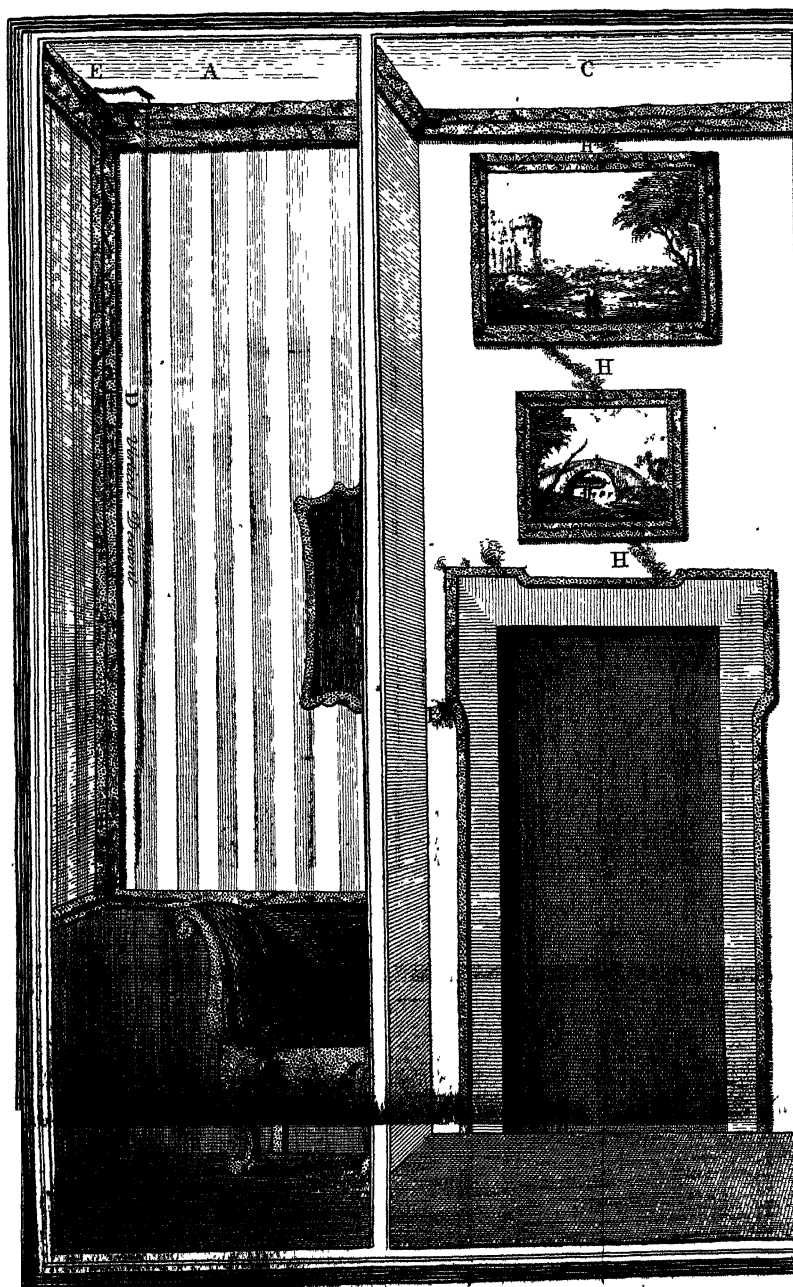
humble servant,

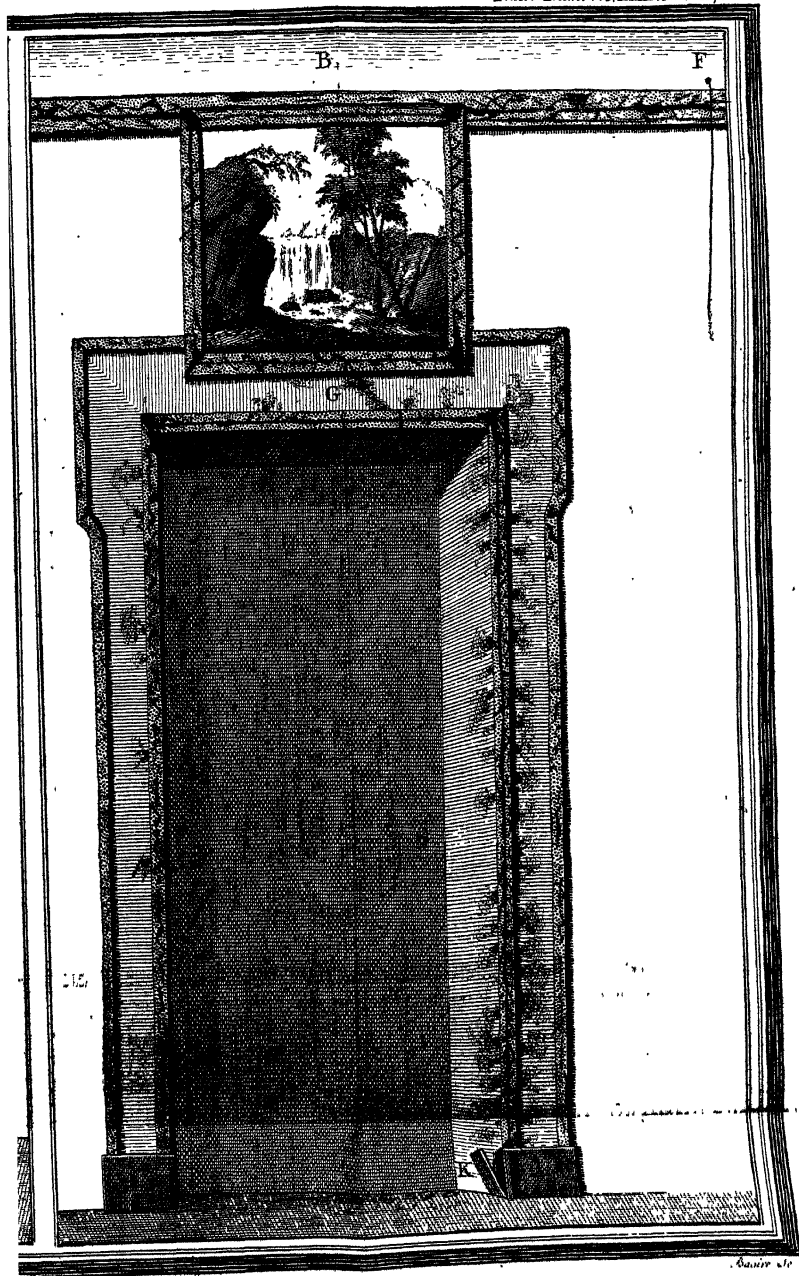
Wm. Hamilton.

Explanation of PLATE XIII.

A. Section of the third room. B. Section of the eighth room. C. Section of the ninth room. D. Bands by which the lightning descended vertically. E. The gilding affected most in the corners and marbled with black, where the lightning was most powerful. F. Bell-wire melted. G. Jump of the lightning traced on the white wall. H. Vertical descents of the lightning marked on the white walls; the greatest jump, from the first picture frame to the second, is 13 inches. I. Hole made in the wall by the lightning. K. Piece of wood beat out of its place and burnt. L. Sofa, the gilding blackened.

Received





Received, April 22, 1773.

XXXV. *An Extract of a Letter from
Dr. Nooth to Dr. Franklin, F. R. S.
on some Improvements in the Electrical
Machine.*

Read June 24, 1773. **I**T must undoubtedly appear extraordinary, that, in the present age, when the study of electricity is become so general, and the advances that have been made in the science are so very considerable, I should attempt to recall your attention to the structure of the electrical machine. But I believe it must be allowed, that, notwithstanding the remarkable progress that has of late been made in electrical pursuits, the machine still remained the most imperfect part of the apparatus. The construction of it has been in general left to the workman, who has seldom been in a capacity of making those improvements in it, which it certainly admits of.

The subject, however, seems well worth the attention of the electricians themselves; as a knowledge of the means of correcting the capricious state of their machines will enable them to pursue

their electrical inquiries with more certainty, success, and satisfaction.

A prospect of discovering the cause of the common uncertainty in the action of electrical machines, induced me, some months since, to make some observations on the appearances produced by a machine in motion. Being well convinced, that the electric fire, which we receive from a machine, is derived from the cushion, and from such parts as communicate with it, I first attended to the effects which the glass and rubber had on each other.

My inquiries, however, were not directed towards an investigation of the cause of that accumulation of electric matter, in consequence of the friction of the glass on the rubber, as I looked on that circumstance as a secret in nature, no less impenetrable than gravitation itself; but I endeavoured to find out the best method of increasing the excitation of a glass, and of taking from it that fire for electrical purposes which might be collected on its surface.

It is evident, that the electric matter is excited in the instant that the glass passes over the rubber, and that it becomes sensible to us by its adhering to the revolving surface of the glass. It likewise appeared to me highly probable, that the quantity of fire, which we find on the glass in motion, is not the whole of that which is excited by the passage of the glass on the rubber. The luminous appearance in the angles between the glass and rubber, and which is extremely distinct in a dark room, rendered it next to certain, that a part of the excited electric fluid returns immediately to the
cushion

cushion without performing a revolution with the glass; and that, of course, a circulation of the fire is thus kept up in the substance of the cushion in the common method of constructing the machines.

To be certainly convinced of this, I attempted to make the passage of the fire from the glass to the anterior part of the cushion, or to that part which corresponds with the ascending side of the cylinder, demonstrable, by placing a piece of silk between the glass and cushion. This silk was larger than the cushion; and part of it was allowed to adhere, by the attraction of the electric fire, to the ascending part of the cylinder. My view in doing this was to cut off, in that part, the immediate communication between the excited glass and cushion, and by that means render the circulation of electric matter visible, which I suspected to take place in the machine; as it was thus forced to turn over the loose edge of the silk before it could return to the cushion. The event answered my expectation; and I then perceived, that the greatest part of the excited fluid was commonly re-absorbed by the fore-part of the cushion without becoming sensible on the superior part of the glass.

Having thus verified my supposition by actual experiments with silken flaps of different sizes, I endeavoured to discover a method of preventing that circulation of the electric fluid, and, if possible, of obliging the whole, or the greater part of it, that is once excited, to make the revolution with the glass. This, indeed, the silk, when of considerable breadth, in some measure effected; but I thought that this obstruction to the immediate re-

turn of the fire might be rendered more complete by increasing the thickness of the silk, or by applying to it some nonconducting substance, that might confine the excited fluid more perfectly to the surface of the revolving cylinder.

Bees-wax being a nonconducting substance easily procured, I rubbed the silken flap with it, and found, as I expected, that the return of the fire to the cushion at the anterior part of the machine was by that means much diminished, and consequently the excitation of the glass was apparently increased. The addition, however, of more silk was still more effectual in confining the fire to the glass; and when it was employed ten or twelve times doubled, it seemed to deny any passage from the glass to the cushion.

As I thus discovered the method of remedying the common defect in the construction of the anterior part of the cushion, I next attended to that part which corresponds with the descending side of the cylinder. Being convinced that this part of the rubber was alone concerned in the excitation, I imagined that the reverse of what was necessary anteriorly should be adopted in the structure of the posterior part; that, instead of placing nonconducting substances between the glass and cushion, we should here make the afflux of the electric matter as great as possible, by the application of the most perfectly conducting bodies. Confining therefore the amalgam to that place where the glass first comes in contact with the rubber, I placed some tinfoil close to the amalgam, and, bending it back, secured it to the metallic plate below the cushion. By this means

means the electric matter found an easy access to the place of excitation; and the effect of the machine was thereby incredibly increased. A piece of leather, covered with amalgam, and fixed to the posterior part of the rubber, in such a manner as to allow about an inch of it to pass under the cylinder, answered every purpose of the tinfoil; and, as it was not liable to be corroded by the mercury, like tinfoil, it was on that account much preferable.

From the above experiments it was apparent that the excitation was altogether performed by the posterior portion of the cushion; and that the anterior part, when made of conducting substances, re-absorbs the greater quantity of the excited matter. In the structure therefore of electrical machine, we should always have a free electric communication behind, to facilitate the excitation; and the most perfectly nonconducting substances before, to prevent the re-absorption. To answer these intentions, it will perhaps be advisable to make the cushion of silk, stuffed with hair, and to lay some metallic conductor round the posterior part, that a free access might be allowed to the electric matter coming to the place of excitation from the inferior part of the machine. Cushions, made in this manner, and then covered with silk ten or twelve times doubled, are much more powerfully excitant than any others that I have yet tried. Various other methods, however, may be pursued in the construction of the rubber; but it should be an invariable rule, to place nonconducting bodies before, and conducting substances behind, the cylinder. From the preceding principles, it follows, that the support

to the rubber should likewise have its conducting and nonconducting side. For this purpose, it may be necessary to employ baked wood, and to cover the posterior half with tinfoil. The place of excitation will be thus sufficiently supplied with electric matter; and the cylinder will not be robbed of a part of the excited fire, before that fire has made a revolution with the glass.

By attending to the place where the excitation is effected, it must appear evident, that the amalgam is only to be laid on the posterior part of the cushion; its presence, indeed, would be useless, if not injurious, in any other situation. It will, however, be found somewhat difficult to confine the pure amalgam to the posterior part of the rubber; but if it is mixed with a little hair-powder and pomatum, it pretty perfectly keeps its place. The strewing the amalgam thus prepared on the glass, as it revolves, is perhaps the best method of applying it; as, by that means, it is in a great measure prevented from passing on to the nonconducting substances that are placed before. Should any of the amalgam be carried forward by the revolution of the glass, it should be carefully removed. The necessity of keeping that part free from conducting bodies cannot be too much insisted on; and, when fresh amalgam is applied as before mentioned to the proper part of the rubber, the flap should be held down during half a dozen turns of the machine, lest it might collect some of the amalgam before it is properly fixed. It is a probable conjecture, that, when the flap of silk is covered with amalgam, part of the amalgam, which is not immediately subservient to the excitation,

tion, acts as a conductor in restoring the fire again to the cushion ; and that thus, by an improper disposition of it, we suppress, instead of increasing, the quantity of the excited matter.

In short, when an electrician attends to the preceding principles in the construction of his rubber, and to the proper disposition of the amalgam, he has nothing to fear from the humidity of the atmosphere, as his machine will work equally well in all kinds of weather. The rest of the electrical apparatus may be made according to the directions that have been given by the different electrical writers. Each has had his favourite machine ; and, perhaps, no one has been yet contrived that has not had its peculiar advantages.

Received May 13, 1773.

XXXVI. *Properties of the Conic Sections; deduced by a compendious Method. Being a Work of the late William Jones, Esq; F. R. S. which he formerly communicated to Mr. John Robertson, Libr. R. S. who now addresses it to the Reverend Nevil Maskelyne, F. R. S. Astronomer Royal.*

S I R,

Read June 24, 1773. **Y**OU well know that the curves formed by the sections of a cone, and therefore called CONIC SECTIONS, have, from the earliest ages of geometry, engaged the attention of mathematicians, on account of their extensive utility in the solution of many problems, which were incapable of being constructed by any possible combination of right lines and circles, the magnitudes used in plane geometry. The properties of these curves are become far more interesting within the two last centuries, since they have been found to be similar to those which are described by the motions of the cœlestial bodies in the Solar system.

Two

Two different methods have been taken by the writers who have treated of their properties; the one, and the more antient, is to deduce them from the properties of the cone itself; the other is to consider the curves, as generated by the constant motion of two or more straight lines moving in a given plane, by certain laws.

There are various methods of generating these curve lines *in plano*; one method will give some properties very easily; but others, with much trouble: while, by another mode of description, some properties may be readily derived, which, by the former, were not so easily come at: so that it appears there may be a manner of describing the curves similar to the Conic Sections, by the motion of lines on a plane, which, in general, shall produce the most essential properties, with the greatest facility.

That excellent mathematician, the late William Jones, Esq; F. R. S. had drawn up some papers on the description of these curves, or lines of the second kind, very different from what he gave in his *Synopsis Palmariorum Matheseos*, published in the year 1706; or from that of any other writer on this subject. A copy of these papers he was pleased to let me take about the year 1740. He had not finished them as he intended; but, in their present state, they appear of too much consequence to be lost; as, it is much to be feared, his own copy, together with many other valuable papers, are; and therefore, I am desirous of preserving them in the Philosophical

Transactions, in the manner I at first transcribed them; although, I am aware, they might have been put into a form more pleasing to the generality of readers: I have indeed annexed larger diagrams than what accompanied the author's copy, in order to render the lines more distinct, as all the relations are to be represented in a single figure, of each kind.

Mr. Jones, having laid down a very simple method of describing these curves, seems to have been desirous of arriving at their properties in as expeditious a way as he could contrive; and therefore he has used the algebraic method, in general, of reducing his equations; and, on some occasions, has used the method of fluxions, to deduce some properties chiefly relating to the tangents; and, by a judicious use of these, he has very much abridged the steps which otherwise he must have taken, to have deduced the very great variety of relations he has obtained: these he intended to have arranged in tables, from whence an equation expressing the relation between any three or more lines of the Conic Sections, might be taken out as readily as a logarithm out of their tables; this he has only partly executed; but it may easily be continued by those who are desirous to have it done, and are sufficiently acquainted with what follows.

From the House of the Royal Society,
April 29, 1773.

THE DESCRIPTION OF LINES OF THE SECOND KIND.

LET the right lines AD , AQ , be drawn on a plane, at any inclination the one with the other. See PLATE XIV. Fig. 1, 2, 3.

In AD , AQ , take Aa , AM , of any given magnitude, and draw MN parallel to AD .

On the points A , a , let two rulers AP , ap , revolve, and cut MN , AQ , in N and Q , so that AQ be every-where equal to MN .

Then shall the intersection P of the rulers describe lines of the second kind, or curves of the first kind.

Where the right-line Aa , is the first, or transverse diameter.

The point c , bisecting the diameter Aa , is the center.

The right-line PD , drawn parallel to AQ , is the ordinate to the diameter Aa .

The part AD , or CD , of the diameter, is the absciss, when reckoned to begin from A to c , or from c to A .

The right line ab drawn from the center c parallel to the ordinate PD , and terminated in the curve, is called the second, or conjugate diameter.

Those diameters to which the ordinates are perpendicular, are called the axes.

And AM is the parameter to the diameter Aa .

THE PROPERTIES OF LINES OF THE SECOND KIND.

1. Put $Aa=d=2AC=2t$; $Bb=d=2BC=2c$; $AM=2p$; $PD=y$; $CD=x$; $AD=u$.

Then $\overline{PD}^2 = \frac{p}{t} \times ADA$.

Or $yy = \frac{p}{t} \times u \times \overline{d \mp u} = 2pu \mp \frac{p}{t} uu = \frac{p}{t} \times \pm tt \mp xx = \pm pt \mp \frac{p}{t} x^2$.

For $PD = \frac{AM \times AD}{MN} = \frac{AQ \times Da}{Aa}$ (by sim. \triangle s). Th. $\overline{PD}^2 = \frac{AM}{Aa} \times ADA$.

2. Consequently $\frac{t}{p} yy = \pm tt \mp xx = du \mp uu = \pm \frac{1}{2} dd \mp xx$.

3. Hence $\frac{t}{p} yy = \mp xx = t \mp u \times u = \mp uu$.

4. And $pt = cc$, or $2pd = dd$; for when $y=c=\frac{1}{2}d$, then $x=0$.

5. Therefore $\frac{\overline{PD}^2}{ADA} = \frac{yy}{\pm tt \mp xx} = \frac{p}{t} = \frac{2p}{d} = \frac{2pd}{dd} = \frac{pt}{tt} = \frac{dd}{dd} = \frac{cc}{tt} = \frac{BC^2}{AC^2}$.

6. The curve line whose property is $yy + \frac{p}{t} uu - 2pu = 0$,

(where the abscissa begins at the curve),

Or $yy + \frac{cc}{tt} xx - cc = 0$, (where it begins at the center),

is called an Ellipsis. This curve returns into itself. For when $x=0$, then $y=c$; and when $y=0$, then $x=t$. Which can happen but two ways.

7. The curve line whose property is $yy - \frac{p}{t} uu - 2pu = 0$,

Or $yy - \frac{cc}{tt} xx + cc = 0$, is called an Hyperbola. This curve spreads out infinitely.

For y increases as x increases; and has four legs tending contrary ways: for xx , or yy , may be produced as well from $-x$, or $-y$; as from $+x$, or $+y$.

8. If the point a , is supposed to be at an infinite distance from A , so that a ruler ap moves in a parallel position to AD ; then is $yy=2pu$, or $yy-2pu=0$, the property of the curve described, and is called a Parabola. This curve spreads out infinitely; for y increases as u increases.

9. Let Aa , Pp , be any two first diameters; Bb , Qq , their second diameters.
 PLATE XIV. Fig. 4.

Draw the ordinates PD , QE , to the diameter Aa , and the ordinate pd , to the diameter Bb .

Let PT be a tangent, and PM be perpendicular, to the curve, in P ; PT cutting Aa , Bb , produced, in T , t ; and PM , in M , m .

Put the subtangent $DT = t$, $dt = \sigma$.

Let $AT = r$, $PM = \pi$, $Ch = w$, $CT = q = a \pm s = t \pm r$.

Put s , s' , = sine and cosine of the angle MPD , or angle PMB .

R = tabular radius.

Then $CDT = \pi s = t \mp u \times s = \frac{t}{p} yy = \frac{tt}{cc} yy = \pm tt \mp \pi\pi = u \times \overline{d \mp u} = ADa$.

For $u \times \overline{t \mp u} = \left(\frac{t}{p} yy \right) = ayj$. Therefore $\left(\frac{u}{j} = \frac{ay}{t \mp u} \right) = \frac{ay}{\pi} = \frac{s}{y}$ (by sim. Δs).

And $cdt = y\sigma = \frac{p}{t} \pi\pi = \frac{cc}{tt} \pi\pi = \pm cc \mp yy = Bdb$. For $\left(\frac{u}{j} \right) = \frac{ay}{\pi} = \frac{\pi}{\sigma}$.

10. Hence $\overline{AC}^2 = tt = \pi \times \overline{\pi \pm s} = \pi \times \overline{t \pm r} = \pi q = DCT$.

11. And $\overline{BC}^2 = cc = y \times \overline{y \mp \sigma} = y \times \overline{ct} = dCt$.

12. Consequently $\frac{\overline{Pd}^2}{Bdb} = \frac{\pi\pi}{\pm cc \mp yy} = \frac{tt}{cc} = \frac{\overline{AC}^2}{\overline{BC}^2}$.

13. Also $\overline{CE}^2 = ww = \left(\overline{DT} \times \frac{\overline{QE}^2}{\overline{PD}^2} = \overline{DT}^2 \times \frac{AEa}{ADA} \right) = ss \times \frac{tt - \pi\pi}{ss} = s\pi = \frac{tt}{cc} yy = ADA = CDT$.

14. Therefore $AEa = \left(\frac{\overline{CE}^2}{\overline{DT}^2} \times ADA = \frac{ss}{ss} \times s\pi \right) = \pi\pi = \overline{CD}^2$.

15. And $\overline{QE}^2 = \left(\frac{\overline{PD}^2}{ADA} \times AEa \right) = \frac{cc}{tt} \pi\pi = \frac{p}{t} \pi\pi$.

In the general schemes. PLATE XV. and Fig. 5. PLATE XIV.

16. Let Aa , Bb , be the longest and shortest axes.

Draw cq perpendicular to the tangent PT , cutting it in q .

Put $cp = T$; $cq = c$; $ce = e$.

$$\text{Then } ce = e = \left(\frac{ct \times ce}{cq} = \frac{ct}{yy} \times \frac{ty}{cc} \right) \frac{tc}{c} = \frac{ac \times cb}{cq}.$$

Hence the parallelogram, under the two axes, is equal to the parallelogram under any two diameters.

17. Draw the tangents AN , an , to any vertices A , a , meeting any diameters $p p$, $q q$, produced in v , u , and v , x , and the tangent PT in N , n .

$$\text{Then } au = \left(\frac{ca \times eq}{ce} \right) \frac{t}{p}. \text{ And } av = \left(\frac{ca \times pd}{cd} \right) \frac{t}{x}.$$

$$18. \text{ Also } cu = \left(\frac{au \times cq}{eq} \right) \frac{cc}{y}. \text{ And } cv = \left(\frac{cp \times ca}{cd} \right) \frac{t}{x}.$$

$$19. \text{ Hence } pv = (cv \oslash cp) \times \frac{t \oslash x}{x}. \text{ And } pv = T \times \frac{t+x}{x}.$$

$$\text{Also } qu = (cu \oslash cq) \times \frac{c \oslash y}{y}. \text{ And } qu = c \times \frac{c+y}{y}.$$

20. When Aa and Bb are the longest and shortest axes; and when $y = p$,

Then $xx = tt \mp \frac{t}{yy}$ will become $tt \mp pt = tt \mp u$, which call ff .

And $cd = x$, will become $cf = cf = f$.

The points F , f , are called the Focii.

$$21. \text{ Hence } af = af = \pm t \mp f; \text{ } af = af = t + f.$$

$$22. \text{ Also } \overline{cf}^2 = \overline{cf}^2 = ff = \pm tt \mp tc = \pm tt \mp pt.$$

$$\text{And in the ellipsis, } \overline{ac}^2 = (\overline{bc}^2 + \overline{cf}^2) = \overline{bf}^2.$$

$$\text{in the hyperbola, } \overline{cf}^2 = (\overline{ac}^2 + \overline{bc}^2) = \overline{ba}^2.$$

Hence, a circle described from B , with the distance ac in the ellipsis, or from c , with the distance ab in the hyperbola, will cut the axis Aa in the focii F , f .

23. Draw

23. Draw FP , fP , from the foci F , f , to any point P of the curve; and draw the conjugate diameters Pp , Qq .

Put $PF = z$; $Pf = v$; $PC = T$; $QC = C$; $\frac{f}{t} = \phi$; $\frac{f}{c} = \gamma$. Then

$$\overline{Pf}^2 = vv = yy + xx + 2xf + ff = tt + 2fx + \frac{ff}{tt}xx = TT + ff + 2fx.$$

$$\overline{Pf}^2 = zz = yy + xx - 2xf + ff = tt - 2fx + \frac{ff}{tt}xx = TT + ff - 2fx.$$

$$\text{For } yy = \left(\frac{cc}{tt} \times \overline{tt \oslash xx} \right) \frac{tt \oslash ff}{tt} \times \overline{tt \oslash xx}.$$

$$24. Pf = v = \left(\sqrt{tt + 2fx + \frac{ff}{tt}xx} \right) t + \frac{f}{t}x = t + \phi x = \frac{tt + fx}{t}.$$

$$PF = z = \left(\sqrt{tt - 2fx + \frac{ff}{tt}xx} \right) t - \frac{f}{t}x = t - \phi x = \frac{tt - fx}{t}.$$

$$25. PF \pm Pf = z \pm v = 2t = Aa.$$

$$26. Pf^2 + \overline{Pf}^2 = vv + zz = 2yy + 2xx + 2ff = 2tt + 2\phi\phi xx = 2TT + 2ff.$$

$$27. \overline{Pf}^2 - \overline{Pf}^2 = vv - zz = 4fx = \overline{v + x} \times \overline{v - x} = 2t \times \overline{v \mp x}.$$

$$28. Fpf = zv = x \times \overline{2t \mp z} = 2tx \mp zz = tt \oslash \phi\phi xx = \overline{tt \oslash xx} \times \frac{cc}{tt}xx$$

$$= cc \oslash \frac{ff}{cc} yy = \frac{tt}{cc} yy + \frac{cc}{tt} xx = \overline{cE}^2 + \overline{EQ}^2$$

$$\therefore \overline{cQ}^2 = CC = T \times P.$$

$$29. \text{Let } m = t - z = v - t = \frac{fx}{t} = \phi x = \pm \frac{1}{2}v \mp \frac{1}{2}z$$

$$= \frac{fx}{\sqrt{cc + ff}} = \frac{x}{t} \sqrt{tt \mp cc} = \sqrt{tt \mp zv}$$

$$= \sqrt{tt \mp cc} = \sqrt{ff + cc - zv}.$$

$$30. \text{Hence } \dot{z} = -\dot{v} = \phi \dot{x} = -\frac{f}{t} \dot{x} = \frac{fy}{px} \dot{y}.$$

$$\text{And } \dot{v} = -\dot{z} = \phi \dot{x} = \frac{f}{t} \dot{x} = -\frac{fy}{px} \dot{y}.$$

$$31. AC = t = \frac{3}{2}z \pm \frac{1}{2}v = \frac{cc}{p} = \sqrt{cc \pm ff} = \frac{fx}{m} = \frac{ca}{\sqrt{cc \oslash yy}} \\ = \frac{c}{y} \sqrt{sx} = \sqrt{x \times x \oslash s} = \frac{y^3 + \sqrt{y^3 + 4p^2 x^2}}{2p} = \frac{yy}{2p} + \sqrt{\frac{y^4}{4p^2} + xx}.$$

$$32. \overline{CD}^2 = \overline{Pd}^2 = \overline{nn} = \left[\frac{tyy}{ccs} \right]^2 = \left[\frac{tyy}{ps} \right]^2 = \pm tt \mp \frac{t}{p} yy = \frac{tt}{cc} \times \overline{cc \mp yy} \\ = \frac{ff+cc}{cc} \times \overline{cc \mp yy} = \pm tt \mp \frac{tt}{cc} yy = \frac{mm}{\phi\phi} = \frac{tt}{ff} mm \\ = \frac{cc+ff}{ff} mm = \frac{tt}{ff} \times \overline{tt \mp zv} = \frac{tt}{ff} \times \overline{tt \mp cc} = \left[\frac{tt}{q} \right]^2 \\ = \frac{tt}{cc} \times \overline{zv} - \frac{tt}{cc} yy = \frac{t}{p} \times \overline{zv} - \frac{t}{p} yy = \frac{tt}{ff} \times \overline{t - z}^2 \\ = \frac{tt}{ff} \times \overline{v - 1}^2 = \left[\frac{tt \mp 1z}{f} \right]^2 = \overline{t \mp u}^2 = \sqrt{tt + \frac{1}{4}ss - \frac{1}{2}1} \\ = \frac{tt}{ff} \times \overline{tt \mp \frac{tt}{cc} \pi\pi} = \frac{t^4}{c^2} \times \frac{ss}{RR} \pi\pi.$$

$$33. \text{And } \dot{z} = -\frac{ty\dot{y}}{p\pi} = -\frac{ty\dot{y}}{cc\pi} = -\frac{t\dot{y}}{qq} = -\frac{t\dot{z}}{f} = \frac{t\dot{v}}{f}.$$

$$34. \overline{PD}^2 = \overline{Cd}^2 = \overline{yy} = \frac{p}{t} s\pi = \frac{cc}{tt} s\pi = \frac{cc}{tt} \times \overline{\pm tt \mp n\pi} = \pm cc \mp \frac{cc}{tt} n\pi \\ = \pm pt \mp \frac{p}{t} n\pi = \frac{2cc}{t} u \mp \frac{cc}{tt} uu = \frac{\pm tt \mp ff}{tt} \times \overline{\pm tt \mp n\pi} \\ = \frac{cc}{tt} \times \overline{zv} - \frac{cc}{tt} n\pi = \frac{cc}{tt} \times \overline{cc} - \frac{cc}{tt} n\pi = \frac{cc}{ff} \times \overline{zv - cc} \\ = \frac{cc}{ff} n\pi = \frac{pt}{ff} n\pi \text{ (putting } n\pi = zv - cc) \\ = \frac{t\pi\pi - c^2}{ff} = cc \mp \frac{tt}{cc} \times \frac{ss}{RR} \pi\pi = \frac{s's'}{RR} \pi\pi.$$

$$35. \text{And } \dot{y} = -\frac{p\pi\dot{z}}{ty} = -\frac{cc\pi\dot{z}}{ty} = \frac{p\pi\dot{z}}{fy} = -\frac{p\pi\dot{v}}{fy}.$$

$$36. \text{Also } \frac{ff}{cc} yy = \overline{zv - cc} = \frac{ff}{tt} \times \overline{tt \mp n\pi} = ff \mp \frac{ff}{tt} n\pi \\ = nn = ff \mp mm = cc - cc.$$

$$\begin{aligned}
 37. \overline{PC}^2 &= TT = xx + yy = xx \pm cc \mp \frac{cc}{tt}xx = tt - ff + \frac{ff}{tt}xx = cc + \phi\phi\Delta\Delta \\
 &= cc + tt\omega zu = cc + mm = cc + tt\omega cc \\
 &= 2p + \phi\phi\Delta\Delta = 2cc + ff\omega zu = 2pt + ff\omega zu.
 \end{aligned}$$

$$38. \text{ And } \dot{T} = \frac{\phi\phi x \dot{x}}{r} = \frac{\phi\phi x \dot{x}}{\sqrt{tp + \phi\phi xx}} = \left(\frac{\phi\phi x}{r} \times -\frac{ty \dot{y}}{px} \right) \frac{ff y \dot{y}}{pt r} = -\frac{ff y \dot{y}}{cc r}.$$

$$39. \text{ Also } \overline{CQ}^2 + \overline{CP}^2 = (cc + tt) = \overline{CB}^2 + \overline{CA}^2.$$

$$\begin{aligned}
 40. \text{ CT} &= q = \frac{tt}{x} = (tt \times \frac{f}{tm}) = \frac{ft}{m} = \frac{ft}{t\omega z} = \frac{ft}{\sqrt{tt\omega zu}} \\
 ct &= \frac{cc}{y} = \frac{cf}{z} = \frac{cf}{\sqrt{zu - cc}}.
 \end{aligned}$$

$$41. \text{ And } \dot{q} = -\frac{q \dot{x}}{x} = -\frac{qq \dot{x}}{tt} = \frac{qq y \dot{y}}{cc x} = \frac{qq y \dot{y}}{pt x} = -\frac{tt \dot{x}}{xx} \text{ (for } tt = qx).$$

$$\begin{aligned}
 42. \text{ AT} &= (\pm CT \mp CA) = \pm \frac{tt}{x} \mp t = \frac{t}{x} \times \pm t \mp x = \pm \frac{ft}{m} \mp t = \pm \frac{ft}{x\omega z} \mp t. \\
 aT &= (CT + Ca) = \frac{tt}{x} + t = \frac{t}{x} \times t + x = \frac{ft}{m} + t = \frac{ft}{x\omega z} + t.
 \end{aligned}$$

$$\begin{aligned}
 43. \text{ FT} &= (\pm CT \mp CF = \pm \frac{ft}{m} \mp f = \pm \frac{ft \mp fm}{m}) = \frac{fz}{t\omega z} = \frac{fz}{m} = \frac{tz}{x}. \\
 fT &= (CT + Cf = \frac{ft}{m} + f = \frac{ft + fm}{m}) = \frac{f\omega}{t\omega z} = \frac{f\omega}{m} = \frac{t\omega}{x}.
 \end{aligned}$$

On Aa describe a circle, draw the tangent TP' , and draw CP' .

Continue DP to P' , and at right angles to TP' , draw FR' ,

$$\text{Then } FR' = \left(\frac{FT}{CT} \times CP' = \frac{tz}{x} \times t \times \frac{x}{tt} \right) z = FP.$$

$$fr' = \left(\frac{fT}{CT} \times CP' = \frac{t\omega}{x} \times t \times \frac{x}{tt} \right) \omega = fP.$$

$$44. \text{DT} = s = \frac{tjy}{px} = \frac{tijy}{ccx} = \frac{\pm t \mp xx}{x} = \frac{tnn}{fm}.$$

$$dt = \sigma = \frac{pxx}{iy} = \frac{ccxx}{iijy} = \frac{cc \mp yy}{y} = \frac{cm}{fn}.$$

$$5. \text{AN} = \left(\frac{ct}{ct} \times \text{AT} \right) = \frac{cm}{nx} \times \overline{t-n} = \frac{c}{n} \times \overline{f-}$$

$$an = \left(\frac{ct}{ct} \times \text{AT} \right) = \frac{cm}{nx} \times \overline{t+n}.$$

$$46. \text{VN} = \left(\frac{PV \times ct}{PC} = \frac{t-n}{t} \times \frac{t}{n} \times \frac{fc}{u} \right) = \frac{fc}{nx} \times \overline{t-n} = \frac{fc}{nm} \times \overline{f-}$$

$$\sqrt{zv} - cc \times \sqrt{it-nv} \quad f - \sqrt{it-nv}$$

$$N = ct = \frac{cc}{y}.$$

$$47. \text{AD} = (\pm AC \mp CD = \pm t \mp \frac{tm}{f}) = \frac{t}{f} \times \overline{f \mp m}.$$

$$aD = (AC + CD = t + \frac{tm}{f}) = \frac{t}{f} \times \overline{f + m}.$$

48. Produce PF, pf, to meet the curve in Π, π;

Draw ΠΔ perpendicular to Aa.

Put FΠ = z', fπ = v', FΔ = x'.

$$\text{Now, FD} = (\pm CF \mp CD) = \pm f \mp n = \pm f \mp \frac{tm}{f} = \frac{tz - cc}{f}$$

$$fD = f + \frac{tm}{f} = \frac{tv - cc}{f}.$$

$$\text{Then FΔ} = x' = (cΔ - CF) = \frac{tm}{f} - f = \left(\frac{t}{f} \times \overline{t-n} - f \right) = \frac{cc - tz'}{f}$$

$$fΔ = f + \frac{tm}{f} = \frac{tv' - cc}{f}.$$

49. From F , f , draw FR , fr , perpendicular to the tangent PR , and cutting it in R , r .

$$FR = \lambda = \frac{FR \times CR}{CT} = \frac{c^2}{c} = \frac{c^2}{\sqrt{2t^2 - z^2}} = c\sqrt{\frac{z}{v}}$$

$$= c\sqrt{\frac{t - \phi v}{t + \phi v}} = \sqrt{\frac{pt^2}{zt - z}} = \frac{z^2}{t}$$

$$fr = \lambda' = \frac{fr \times CR}{CT} = \frac{cv}{c} = \frac{cv}{\sqrt{2t^2 - z^2}} = c\sqrt{\frac{z}{v}}$$

$$= c\sqrt{\frac{t + \phi v}{t - \phi v}} = \sqrt{\frac{pt^2}{2t - v}}$$

$$\lambda = \frac{tc^2}{v\sqrt{2v}}. \text{ For } \lambda^2 = \frac{c^2 z}{v}. \text{ Th. } 2\lambda\lambda' = \left(\frac{c^2 v^2 - c^2 z^2}{vv} \right) = \frac{2c^2 t}{v}.$$

$$50. TR = \left(\frac{AT \times RT}{AN} = \frac{tnz}{mc} \right) \frac{ttzn}{f^2 n} = \frac{ttzn}{f^2 n} \sqrt{\frac{z}{v}} = \frac{tn}{m} \sqrt{\frac{z}{v}}.$$

$$Tr = \frac{at \times rf}{an} = \frac{tn}{m} \sqrt{\frac{v}{z}}.$$

51. Draw PM perpendicular to the tangent PT , meeting the axes Aa , ab , in M , m .

$$PM = \left(\frac{PD \times PD}{TD} \right) = \frac{ccm}{ft} = \frac{cc}{ft} \sqrt{tt \pm zv} = \frac{cc}{tt} n = \frac{p}{t} n$$

$$= \frac{s}{r} \tau = p \mp \frac{p}{t} u = \frac{cc}{tf} \sqrt{tt \mp \frac{tt}{cc} \pi \pi}$$

$$dm = \left(\frac{PD \times PD}{DM} \right) = \frac{ttm}{fc} = \frac{tt}{cc} \sqrt{zv - ct} = \frac{tt}{ct} y = \frac{t}{p} y.$$

$$52. CM = (CD \mp DM = n \mp \frac{cc}{tt} n) = \frac{tt \mp cc}{tt} n = \frac{ff}{tt} n = \frac{f}{t} n.$$

$$= \frac{f}{t} \sqrt{tt \mp zv} = \frac{f}{t} \times t \mp z = \frac{f}{t} \times \sqrt{tt \mp \frac{tt}{cc} \pi \pi}$$

$$cm = (dm \mp cd = \frac{tt}{ct} y \mp y) = \frac{tt \mp cc}{ct} y = \frac{ff}{ct} y = \frac{f}{c} n = \frac{f}{c} \sqrt{zv - ct}$$

$$53. FM = (CF \pm CM = f \pm \frac{f}{t} m) = \frac{f}{t} \times t \mp m = \frac{fz}{t} = \pm f \mp \frac{ff}{tt} n.$$

$$fm = (fc \pm CM = f \pm \frac{f}{t} m) = \frac{f}{t} \times t \mp m = \frac{fz}{t} = f \mp \frac{ff}{tt} n.$$

$$54. \overline{fm} = \overline{f^2 m^2} = (\overline{f^2} + \overline{cm^2}) = ff + \frac{ffnn}{cc} = \frac{ffzv}{cc} = \frac{ff}{cc} \times cc.$$

$$55. AM = (AC \oslash CM =) t \oslash \frac{fm}{t} = \frac{tt \oslash fm}{t} = t \oslash \frac{ff}{tt} x.$$

$$am = (ac + cm =) t + \frac{fm}{t} = \frac{tt + fm}{t} = t + \frac{ff}{tt} x.$$

$$56. TM = (TF + FM =) \frac{fz}{m} + \frac{fz}{t} =) \frac{fzv}{tm} = \frac{zv}{n} = \frac{cc}{n}$$

$$tm = (ct + cm =) \frac{fc}{n} + \frac{fn}{c} =) \frac{fzv}{cn} = \frac{zv}{y} = \frac{cc}{y}.$$

$$57. PM = \pi = \left(\frac{PR \times TM}{FT} = \right) \frac{c}{t} \sqrt{zv} = \frac{c}{t} c = \frac{p}{c} c = \frac{c}{t} \sqrt{tt \oslash \phi^3 n^2}$$

$$= \frac{c}{t} \sqrt{t + \phi x \times t - \phi x} = \frac{cc}{tt} \times \frac{R}{s} x = \frac{cR}{ts} \sqrt{ce \mp yy}$$

$$= \frac{c}{tt} \sqrt{t^2 - ffxx} = \frac{c}{t} \sqrt{cc + \gamma\gamma yy} = \frac{1}{t} \sqrt{c^4 + ff\gamma\gamma}$$

$$= \frac{c}{t} \sqrt{2tz \mp zz} = \sqrt{2pz \mp \frac{p}{t} zz}.$$

$$pm = \pi' = \left(\frac{PM \times Pd}{DM} = \right) \frac{t}{c} \sqrt{zv} = \frac{t}{c} c = \frac{c}{p} c = \frac{t}{c} \sqrt{tt \oslash \phi \phi nn}$$

$$= \frac{t}{c} \sqrt{t + \phi x \times t - qn} = \frac{1}{c} \sqrt{t^2 \oslash ffxx} = \frac{t}{c} \sqrt{cc + \gamma\gamma yy}$$

$$= \frac{t}{cc} \sqrt{c^4 + ff\gamma\gamma} = \frac{t}{c} \sqrt{2tz \mp zz} = \sqrt{\frac{2tt}{p} z \mp \frac{t}{p} zz}.$$

$$58. Mm = (pm \mp PM = \frac{t}{c} c \mp \frac{c}{t} c =) \frac{t \mp \frac{cc}{t}}{ct} c = \frac{ff}{ct} \sqrt{zv}.$$

$$= \frac{ff}{ct} \sqrt{tt \oslash \phi \phi nn}.$$

$$59. PT = \tau = \left(\frac{PD \times PM}{DM} = \right) \frac{n}{m} c = \sqrt{\frac{zv - cc}{tt - zv}} \times zv = \frac{tn}{f^x} c.$$

$$= \frac{ty}{cx} c = \frac{ty}{cn} \sqrt{tt \oslash \phi \phi nn} = \frac{\sqrt{zv - cc \times zv}}{t \mp z}.$$

$$pt = \tau' = \left(\frac{PT \times Pd}{DT} = \right) \frac{m}{n} c = \sqrt{\frac{tt - zv}{zv - cc}} \times vz = \frac{fx}{tn} c$$

$$= \frac{cx}{ty} c = \frac{cx}{ty} \sqrt{tt - \phi \phi nn}.$$

$$60. \text{TN} = \left(\frac{\text{PT} \times \text{TA}}{\text{DT}} = \frac{n}{m} \times \frac{t}{m} \times \sqrt{fom} \times \frac{f}{mn} = \right) \frac{f}{mn} \times \sqrt{fom} = \frac{c}{xy} \times \sqrt{fom}$$

$$\text{TN} = \left(\frac{\text{PT} \times \text{Ta}}{\text{DT}} = \frac{n}{m} \times \frac{t}{m} \times \sqrt{f+m} \times \frac{f}{mn} = \right) \frac{f}{mn} \times \sqrt{f+m} = \frac{c}{xy} \times \sqrt{f+m}$$

$$61. \text{PN} = \left(\frac{\text{AD} \times \text{PT}}{\text{DT}} = \frac{t}{f} \times \sqrt{fom} \times \frac{n}{m} \times \frac{f}{mn} = \right) \frac{c}{n} \times \sqrt{fom} = \frac{c}{y} \times \sqrt{fom}$$

$$\text{PN} = \left(\frac{\text{AD} \times \text{PT}}{\text{DT}} = \frac{t}{f} \times \sqrt{f+m} \times \frac{n}{m} \times \frac{f}{mn} = \right) \frac{c}{n} \times \sqrt{f+m} = \frac{c}{y} \times \sqrt{f+m}$$

$$62. \text{PR} = \left(\frac{\text{PT} \times \text{TM}}{\text{TM}} = \right) \frac{n}{c} = n \sqrt{\frac{z}{v}} = \sqrt{\frac{z}{v} \times z^2 - c^2} = \frac{f}{c} y \times \sqrt{\frac{z}{v}}$$

$$= \frac{f}{c} y \times \sqrt{\frac{t - \phi x}{t + \phi x}} = \frac{f}{cc} \times \lambda y$$

$$\text{PR} = \left(\frac{\text{PT} \times f}{\text{TM}} = \right) \frac{nv}{c} = n \sqrt{\frac{v}{z}} = \sqrt{\frac{v}{z} \times z^2 - c^2} = \frac{f}{c} \times y \sqrt{\frac{v}{z}} = \frac{f}{c} y \times \sqrt{\frac{t + \phi x}{t - \phi x}}$$

$$63. \text{Tg} = \left(\frac{\text{PT} \times \text{TC}}{\text{TM}} = \right) \frac{ntt}{mc} = \frac{tt}{t-z} \sqrt{\frac{zv - c^2}{vz}} = \frac{fyt}{mcc} = \frac{y^2}{zc}$$

$$64. \text{Tg} = (\text{Tg} \mp \text{TP} = \frac{ntt}{mc} \mp \frac{nc}{m}) \times \frac{n}{mc} \times t \mp c = \frac{n}{mc} \times nm = \frac{nm}{c}$$

$$65. \text{Ti} = \left(\frac{\text{PT} \times \text{Ci}}{\text{PD}} = \frac{n}{m} \times \frac{c}{n} \times \frac{f}{cn} = \right) \frac{ffc}{mn} = ff \sqrt{\frac{t - zv \times zv - c^2}{t - zv \times zv - c^2}}$$

$$= \frac{tc}{xy} = \frac{tc}{xy} \sqrt{\frac{t - \frac{f}{t} x}{t - \frac{f}{t} x}}$$

$$66. \text{Nn} = \left(\frac{\text{PT} \times \text{Aa}}{\text{DT}} = \right) \frac{2fc}{cn} = \frac{2c}{y} = \frac{2c}{y} \sqrt{\frac{zv - c^2}{zv - c^2}} = 2f \sqrt{\frac{zv - c^2}{zv - c^2}}$$

From c and F, draw cy and v₂, perpendicular to pf
Put v₂ = b; Σ = co-line of the ∠ A f v; R = tabular radius,

$$\text{Then } f y = \left(\frac{cf \times f n}{pf} = \frac{zv - c^2}{f} = \right) \frac{zv - c^2}{v} = \left(\frac{\Sigma}{R} \times c f \right) \frac{\Sigma}{f}$$

$$\text{And } v f = v = \frac{c}{\Sigma} = \frac{t - f f}{t - \frac{\Sigma}{R} f}$$

$$v = b = \left(\frac{\pm xz + uv \mp 4ff}{2v} = \frac{\pm TT \mp ff}{v} = \frac{\pm 2pt \mp vz}{v} \right) \\ = \pm \frac{2pt}{v} \mp z = \pm \frac{x+v}{v} p \mp z,$$

Hence $p = \frac{\pm b \mp z}{x+v} \times v$; $v = \frac{pz}{\mp b \pm z - p}$; $z = \frac{p-b}{v-p} \times v$

Therefore p is less than, equal to, or greater than $\pm b \mp z$, in the Ellipsis, Parabola, or Hyperbola.

67. Draw $m\mu$, making the $\angle Pm\mu = \angle PCM$, and cutting PC in μ .

$$\text{Then } P\mu = \left(\frac{PM \times Pm}{PC} \right) \frac{CC}{T} = \frac{zv}{\sqrt{tt \pm cc \mp zv}} = \frac{tt - \phi\phi xx}{\sqrt{cc + \phi\phi xx}} \\ = P = \frac{1}{2} \text{ parameter to } PC.$$

68. Let $F\phi$ be an ordinate to the axis Aa , at the focus F , and ϕG a tangent to the curve in ϕ , meeting Aa , BC , AN , an , in G , g , s , i . Then

$$F\phi = \sqrt{\frac{cc}{tt} \times AFa} = \sqrt{\frac{cc}{tt} \times cc} = \frac{cc}{t} = \frac{pt}{t} = p \\ = \frac{1}{2} \text{ parameter to } Aa.$$

$$69. CG = \frac{tt}{f} = \frac{ff \pm cc}{f} = \frac{tt}{\sqrt{tt}} \frac{tt}{cc} = \frac{tt}{t \times t - p}$$

$$70. FG = (CG \oslash CF = \frac{tt}{f} \oslash f = \frac{tt \oslash ff}{f} = \frac{cc}{f} = \frac{pt}{f}.$$

71. Draw FH parallel to Aa ; and GH perpendicular to Aa , meeting, FH , FT , in H , h ; then

$$FH = DG = (CG \oslash DC = \frac{tt}{f} \oslash z = \frac{z}{f} \times PF.$$

$$72. \text{TE} = (\text{CT} \propto \text{CG} = \frac{f^t}{m} \propto \frac{t^t}{f} = \frac{t}{m} \times \overline{f \omega x} =) \frac{t}{t-z} \times \frac{\pm t z \mp c c}{f} = \frac{t^t}{f^x} \times \overline{f \omega x}.$$

$$73. \text{Gh} = \left(\frac{\text{PD} \times \text{TG}}{\text{DI}} = \frac{c n}{f} \times \frac{t}{m} \times \overline{f \omega x} \times \frac{f m}{t n n} = \frac{c}{n} \times \overline{f \omega x} = \right) \\ \frac{c c}{f y} \times \overline{f \omega x} = \frac{c}{\sqrt{z v - c c}} \times \frac{t z - c c}{f}.$$

$$74. \text{Ph} = \left(\frac{\text{TP} \times \text{DG}}{\text{DI}} = \right) \frac{z c}{n} = \frac{c z}{f y} = z \times \sqrt{\frac{z v}{z v - c c}} = \frac{c z}{f y} \sqrt{z v}.$$

$$75. \text{Rh} = (\text{Ph} - \text{PR} =) \frac{z c}{n} - \frac{z n}{c} = \frac{z}{n c} \times \overline{c c - n n} =) \\ \frac{z c c}{n c} - \sqrt{\frac{z c c}{z v - c c + z v}} = \frac{z c^3}{f y c}$$

$$76. \text{Rb} = \left(\sqrt{\text{PR}^2 + \text{Rh}^2} = \right) \frac{c z}{n} = \frac{c z}{\sqrt{z v - c c}} = \frac{c c z}{f y} = \frac{p t z}{f y}.$$

77. Let $\text{D}\Sigma$ be any Ordinate to the axe Aa , cutting the curve in Σ , and the focal tangent ΦG in σ ;

$$\text{Then } \text{D}\sigma = \left(\frac{\text{r}\Phi + \text{DG}}{\text{IG}} = \frac{c c}{t} \times \frac{t z}{f} \times \frac{f}{c c} = \right) z = \text{FP} = \text{F}\Sigma.$$

78. Therefore $\text{As} = \text{AF}$; $as = a\text{F}$; $\text{cg} = \text{CA}$; by sim. Δ s.

$$79. \overline{\text{D}\Sigma^2} = (\overline{\text{D}\sigma^2} - \overline{\text{D}\Sigma^2} = \overline{\text{D}\sigma} + \text{D}\Sigma \times \overline{\text{D}\sigma} - \overline{\text{D}\Sigma} =) \text{P}\sigma \times \sigma \Sigma.$$

79. Let the tangents PN , $p\text{L}$, to the opposite vertices P , p , cut the tangents AN , $a\text{n}$, to the opposite vertices A , a , in N , n , L , l .

Then $\text{Pn} = p\text{L}$; $a\text{n} = \text{AL}$; $\text{PN} = p\text{L}$; $\text{AN} = a\text{l}$.

For the Trapezia's pcan , $p\text{CAL}$, are similar and equal;

And so are the Trapezia's PCAN , $p\text{CAL}$.

$$81. PF \times Mf = \left(\frac{f}{t} \times uz = \right) Pf \times MF = \frac{f}{t} \times FP f.$$

$$82. CEQ = (xy =) CDF = dCD = DM \times dm.$$

$$83. AN \times aD = \frac{ctn}{f} = an \times AD = AC \times PD = Dal = DAL.$$

$$84. RPr = nn = TPc.$$

$$85. RT r = \left(\frac{nn}{mm} \times tt = \frac{tt - xx}{xx} \times tt = \right) ATa.$$

$$86. RT f = \left(\frac{tnz}{mc} \times \frac{fv}{m} = \frac{ft}{m} \times \frac{nc}{m} = \right) CTF = r TR.$$

$$87. NTN = \left(\frac{ffcc}{mmnn} \times \overline{ffommm} = \right) \frac{ffcc}{mm} = PTF.$$

$$88. PN \times CB = \left(\overline{tcm} = \frac{fc}{tn} c = \right) AN \times CQ.$$

$$89. CAV = \left(t \times \frac{ty}{x} = \frac{tt}{x} \times y = \right) CT \times PD.$$

$$90. PRb = \left(\frac{cczz}{cc} = \right) \overline{FR}^2.$$

Hence Fb is perpendicular to FB .

$$91. FMf = \left(\frac{fz}{t} \times \frac{fv}{t} = \right) \frac{ff}{tt} \times zv = \frac{ff}{tt} \times cc = \frac{ff}{tt} \times \overline{CQ}^2$$

$$92. AMa = \left(tt \times \frac{ffmm}{tt} = \right) cc \times \frac{ff}{tt} \times zv$$

$$\therefore \overline{AM}^2 = \frac{ff}{tt} \times \overline{CQ}^2 = \overline{CP}^2 + FMf.$$

$$93. \frac{\overline{PN}^2}{\overline{TN}^2} = \frac{x x}{t t} = \frac{\overline{CD}^2}{\overline{AC}^2} = \frac{\overline{AN}^2}{\overline{VN}^2} = \frac{\overline{CA}^2}{\overline{CT}^2} = \frac{\overline{AD} a}{\overline{AT} a} = \frac{\overline{AD} a}{\overline{RT} r} = \frac{\overline{AD}^2}{\overline{AT}^2} = \frac{\overline{aD}^2}{\overline{aT}^2} = \frac{\overline{CDT}}{\overline{AT} a},$$

$$94. \frac{\overline{PT}}{\overline{CT}} = \frac{x}{t} = \frac{\overline{TP}}{\overline{CA}} = \frac{\overline{PR}}{\overline{C\phi}}. \text{ Sim. } \triangle s \text{ TFR, TC}\phi.$$

$$\text{And } \frac{\overline{AD}}{\overline{DT}} = \frac{x}{t+x} = \frac{\overline{CD}}{\overline{aD}} = \frac{\overline{AC}}{\overline{aT}}.$$

$$95. \frac{\overline{fT}}{\overline{CT}} = \frac{p}{t} = \frac{\overline{fP}}{\overline{AC}} = \frac{\overline{fR}}{\overline{C\phi}}. \text{ And } \frac{\overline{AT}}{\overline{DT}} = \frac{t}{t+x} = \frac{\overline{AC}}{\overline{aD}} = \frac{\overline{CT}}{\overline{aT}}.$$

$$96. \frac{\overline{PM}}{\overline{fm}} = \frac{cc}{t t} = \frac{p}{t} = \frac{\overline{BC}^2}{\overline{AC}^2}. \text{ And } \frac{\overline{PM}}{\overline{fm}} = \frac{t}{f} = \frac{\overline{AC}}{\overline{CF}}.$$

$$97. \frac{\overline{PMf}}{\overline{PM}^2} = \frac{\overline{ff}}{\overline{cc}} = \frac{\overline{t+c} \times \overline{t-c}}{\overline{cc}} = \left(\frac{t t - c c}{t p} \right) \frac{t-p}{p} = \frac{\overline{CF}^2}{\overline{CB}^2}.$$

98. Let PM , the perpendicular to the tangent PT , cut the axis Aa in M ; and fP , produced, cut FR in ϕ .

Then will PM bise ϕ t the angle FPf .

For $PF \times Mf = Pf \times MF$.

99. And the angle FPT is equal to the angle fPt .

For $\angle TPF + \angle FPM = \angle tPf + \angle fPM$.

100. Therefore PT will bise ϕ t the angle $FP\phi$.

For $\angle \phi PT = (\angle fPt =) \angle FPT$.

101. Consequently $P\phi = PF$. And $R\phi = RF$.

102. A circumference of a circle described from t , with the radius tN , will cut the axis Aa in the foci F, f .

For $TN \times TN = TF \times Tf$.

103. A circumference of a circle described from c , with the radius ca , will cut the tangent PT in R , r .

Whence the perpendiculars RE , rf , to that tangent, will cut the axis Aa in the foci F , f .

For $TR \times Tr = TA \times Ta$.

104. A circumference described from B , with AC , in the Ellipsis, or from c , with AB in the Hyperbola, will cut the axis Aa , in the foci F , f .

For, in the Ellipsis, $tt = cc + ff$, or $\overline{AC}^2 = (\overline{BC}^2 + \overline{CF}^2) = \overline{BF}^2$.

And in the Hyperbola, $ff = tt + cc$, or $\overline{CF}^2 = (\overline{AC}^2 + \overline{BC}^2) = \overline{AB}^2$.

105. Let cQ produced, cut PF , Pf , in z , n ; draw mz , mx , and MZ , MX , parallel to them, cutting PF , Pf in z , x .

Then $Px = CR = CA = Pz = t$.

106. Hence $\angle Pxz = \angle Pzn$; and $\angle mxz = \angle mzn$.

For Pm is perpendicular to zx .

Consequently, the angles Pzm , Pxm ; PZM , PXM are equal.

107. And the triangles Pzm , Pxm , are similar and equal:

And so are the triangles PZM , PXM .

Consequently, the trapezias $Pzmn$, $PZMX$, are similar.

108. Let CR , cr , cut PF , Pf , in K , k .

Then $CK = \left(\frac{CF \times Pf}{Pf} \right) = \frac{1}{2} Pf = Pk = kr$.

And $ck = \left(\frac{cf \times PF}{Pf} \right) = \frac{1}{2} Pf = PK = KR$.

109. Also $Px = Pz = \left(\frac{PM}{Pm} \times Pz = \frac{cc}{t} \times \frac{t}{tc} \times t \right) = \frac{cc}{t} = \left(\frac{pt}{t} \right) = p$.

110. The Trapezias $FOBR$, $PDFR$, $frtc$, are similar, and consequently their corresponding parts are proportional.

$$\text{That is, } \left\{ \begin{array}{l} \frac{FG}{PD} = \frac{Gh}{DR} = \frac{BR}{IR} = \frac{RP}{RP} \\ \frac{fr}{ir} = \frac{it}{ic} = \frac{cf}{cf} \end{array} \right\}$$

For the triangles Rfb , RPF , and fbG , FPD , are similar.

111. The Trapezias $cePD$, $tCMF$ are similar, and consequently their corresponding parts are proportional.

$$\text{That is, } \frac{ce}{tc} = \frac{ep}{cm} = \frac{PD}{PM} = \frac{DC}{PF}.$$

112. And CR , cr , are parallel to pf , PF , and equal to CA .

$$\text{For } RTf = CTP = rTF.$$

113. Let $\Sigma = \text{fine of the } \angle tPf$, or TPF ; $R = \text{tabular radius}$.

$$\text{Then } \frac{R}{\Sigma} = \left(\frac{pf}{fr} \right) = \frac{c}{c} = \frac{\sqrt{2}v}{c}.$$

Put $\Sigma = \text{fine of the } \angle PCQ$, made by any diameter and its ordinate.

$$\text{Then } \frac{PT}{c} = \frac{1}{2}ff \pm \sqrt{\frac{1}{4}f^4 \pm \frac{r^2 t^2}{\Sigma^2}}; \quad \frac{tt}{cc} = \frac{1}{2}fr \pm \sqrt{\frac{1}{4}f^4 \pm \frac{r^2 c^2}{\Sigma^2}}.$$

114. Let the parallels $Pf\Pi$, $c\beta r$, p/π be drawn, cutting the curve in Π , β , π ; and ordinately applied to some diameter (2τ), whose parameter is 2π , and conjugate $c\beta = x$, to which $r\pi$ is ordinately applied at δ .

$$\text{Then } c\delta = \left(\frac{PF + \pi f}{2} = \frac{PF + \Pi f}{2} \right) = \frac{1}{2}x + \frac{1}{2}x = \frac{1}{2}P\Pi = \left(\frac{c\beta^2}{cr} \right) = \frac{(\beta^2)}{\Delta C} = \frac{x\pi}{t} = \frac{\pi\tau}{t}.$$

$$115. \overline{c\beta^2} = \left(\frac{1}{2}AC \times P\Pi \right) = \frac{1}{2}t \times \overline{c\beta^2} = \left(AC \times \frac{P\Pi}{\Delta C} \right) = \frac{tt}{cc} x\pi = \frac{t}{p} x\pi.$$

$$116. \frac{r\pi}{P\Pi} = \frac{x}{x} = \frac{PD}{\Delta C} = \frac{tx - cc}{cc - t\omega} = \frac{x - p}{p - x}.$$

$$117. P\Pi = \frac{p\pi x}{2x - p} = \frac{1}{2}p \times \overline{x + x} = \frac{1}{2}p \times P\Pi.$$

$$118. P\Pi = x + x = \frac{2x\pi}{2x - p} = \frac{2\pi\tau}{t} = \frac{\tau}{t} \times 2\pi.$$

$$\overline{CB}^2 = cc = pt = \pm tt \mp ff = \pm t \mp f \times t + f = Afa = VAU = tcd = CFG$$

$$= PM \times Cg = FR \times fr = AN \times an = al \times AL = AC \times F\Phi = tt \times \frac{PM}{PM}$$

$$= tt \times \frac{DM}{DC} = tt \times \frac{AC}{dm} = tt \times \frac{r^2}{CQ^2} = tt \times \frac{PD^2}{ADa} = tt \times \frac{r^2}{TDC} = ff \times \frac{1}{cm}$$

$$: AMa - rMf = FPF - RPr = ff \times \frac{t}{r}$$

PN

$$= TT + CC - tt = \frac{tt}{xx} \times \overline{zu + xx} - tt = \frac{1}{2} zu \pm \sqrt{\frac{1}{4} z^2 u^2 ff}.$$

$$\overline{CQ}^2 = CC = P \times T = MPm = NPN = TPl = FPF = CP\eta = Lpl$$

$$= PN \times Pl = CD \times IM = PD \times tm = \frac{tt}{cc} \times PM^2 = \frac{cc}{tt} \times PM^2 = \frac{cc}{ff} \times fm^2$$

$$= \frac{tt}{ff} \times FMf = \left[\frac{ct}{ff} \times Mm \right]^2 = \left[\frac{ACB}{Cg} \right]^2 = PM^2 + FMf = BC^2 + RPr$$

$$= tt + cc - TT = tt - \frac{ff}{tt} \times xx = \frac{tt}{cc} \times yy + \frac{cc}{tt} \times xx = \frac{DC}{DT} \times PT^2.$$

$$\overline{Ck}^2 = ff = tt - cc = tt - tp = \frac{cc}{p} \times t - p = \frac{tt}{xx} \times mm = P\eta \times Tl$$

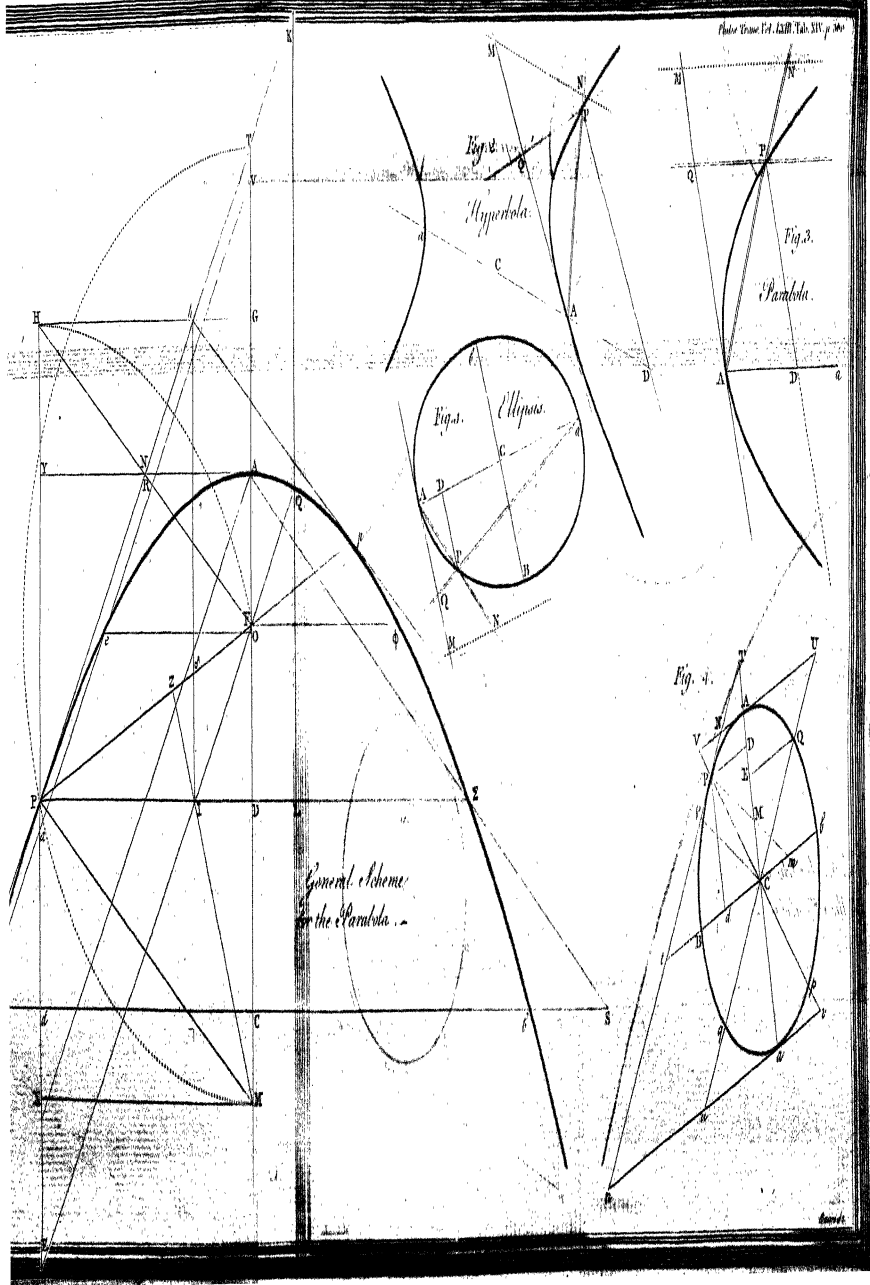
$$= \frac{tt}{cc} \times FMf = \frac{cc}{cc} \times fm^2 = \frac{mm}{cc} \times FTf = \frac{mm}{cc} \times NTn.$$

The semi-parameter (p) to the greater axis (Aa) is equal to

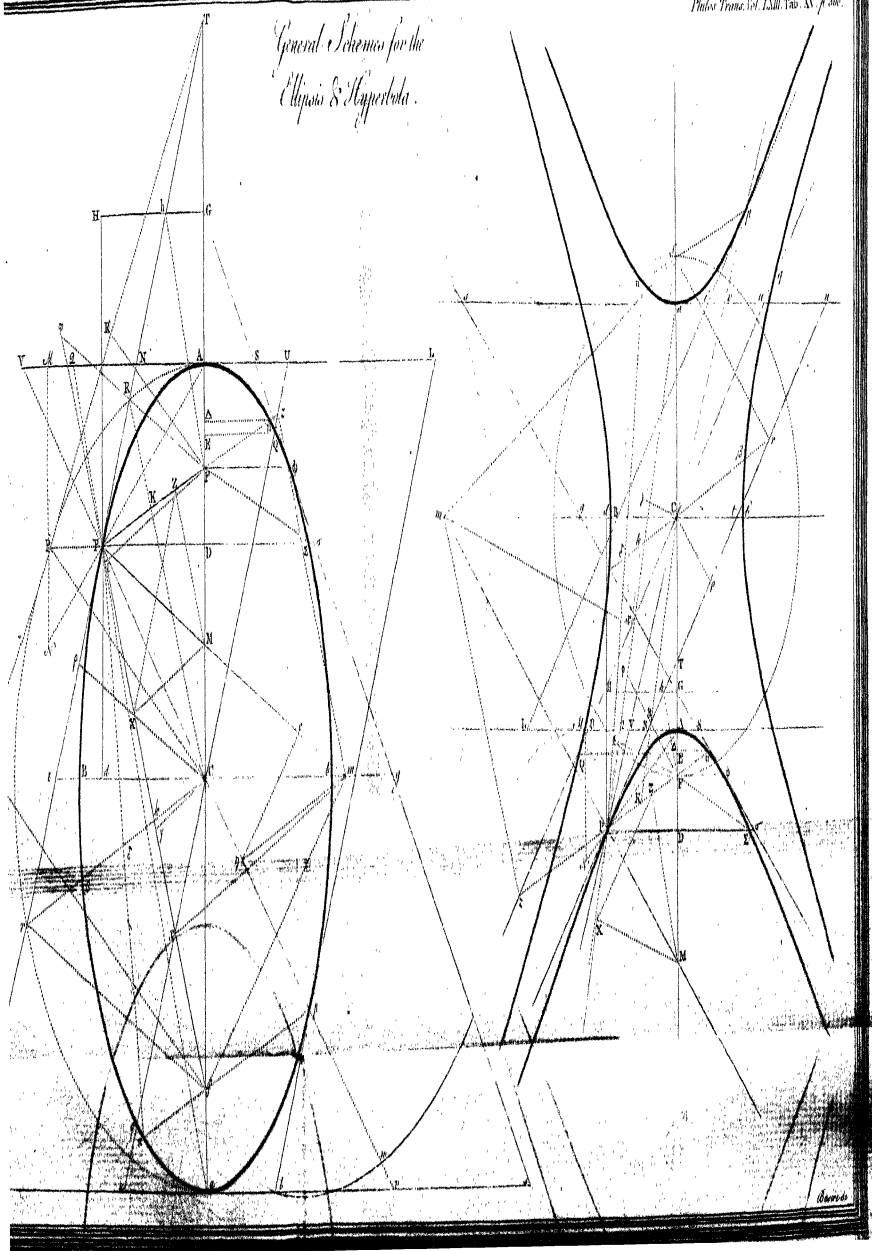
$$\begin{aligned} F\Phi = PZ &= \frac{Afa}{AC} = \frac{VAU}{AC} = \frac{CFG}{AC} = \frac{tcd}{AC} = \frac{PF\eta}{\frac{1}{2}P\eta} = \frac{PM \times Cg}{AC} \\ &= \frac{FR \times fr}{AC} = \frac{AN \times an}{AC} = \frac{AC \times PM}{pm} = \frac{AC \times DM}{DC} = \frac{AQ \times dC}{dm} = \frac{BC \times PM}{CQ} \\ &= \frac{BC \times CQ}{pm} = \frac{BC^2}{AC} = \frac{cc}{t} \times \frac{yy}{xx} = \frac{\pm tt \mp ff}{t} = \frac{cc - yy}{xx} \times t \\ &= \frac{CC + TT - tt}{t} = \frac{CC + xx - tt}{xx} \times t = \frac{2t - x}{tx} \times FR = \frac{2t - x}{ty} \times \overline{fr}^2. \end{aligned}$$

The semi-parameter (p) to any diameter ($p\phi$) is equal to

$$\begin{aligned} P\eta &= \frac{CQ^2}{PC} = \frac{CC}{T} = \frac{MPm}{PC} = \frac{NPN}{PC} = \frac{TPl}{PC} = \frac{FPf}{PC} = \frac{Lpl}{PC} = \frac{PN \times Pl}{PC} \\ &= \frac{CD \times TM}{PC} = \frac{PD \times tm}{PC} = \frac{PT \times PN}{PV} = \frac{tt}{cc} \times \frac{PM^2}{PC} = \frac{cc}{tt} \times \frac{PM^2}{PC} \\ &= \frac{cc}{ff} \times \frac{fm^2}{PC} = \frac{tt}{ff} \times \frac{PMf}{PC} = \frac{tt + cc - TT}{T}. \quad [\text{See TAB. XIV, XV.}] \end{aligned}$$



General Scheme for the
Ellipsis & Hyperbola.



Algebra

XXXVII. *An Essay, towards elucidating the History of the Sea-Anemonies, by Abbé Dicquemare, Member of several Academies, Professor of Experimental Philosophy, &c. at Havre de Grace. Communicated by Matthew Maty, M. D. Sec. R. S. Translated from the French.*

Read Feb. 11,
and April 29,
1773.

Whatever is relative to animals, their mode of being, growth, and decrease, propagation, strength, actions, distempers, food, length of life, and even the phenomena observed at their death, are so many objects interesting to man. If his moral qualities bear no analogy with theirs, his natural constitution admits of similarities between them. •

Memoire pour servir à l'histoire des Anemones de Mer; par Mr. l'Abbé Dicquemare, de plusieurs Académies Royales, Professeur de Physique Experimentale, au Havre de Grace.

TOUT ce qui a rapport aux animaux, leur maniere d'être, les accroissèmens et décroissèmens qu'ils éprouvent, leur génération, leur force, leurs actions, leurs maladies, leur nourriture, la durée de leur vie, les phénomènes qu'ils nous présentent même à la mort, sont autant d'objets qui doivent intéresser l'homme. Si son être moral n'offre avec eux aucune analogie, sa constitution physique permet des similitudes.

Comparative anatomy, and experiments made upon living animals, have unravelled to us part of their vital, natural, and animal faculties. Among the last, those which depend upon the organic disposition of the parts, and which perhaps are the principle of many more functions, might receive new light, from observations to be made upon such animal, as seem to deviate most from our mode of being. Surprized by the phenomena these animals afford, we pry with eager looks into the secrets of nature; there we perceive effects, which shew us, that the inmost conformation of some particular animals not tallying with our former notions, we must renounce, as groundless prepossessions, such laws as we had been too hasty in laying down as universal. Those effects, and the method of observing them, which ought, seemingly, upon a first inspection, to enable

us,

L'Anatomie comparée, et les expériences faites sur les animaux vivans, nous ont dévoilé une partie des fonctions vitales, naturelles, et animales. Parmi ces dernières, celles qui dependent de la disposition organique des parties et qui sont peut-être le principe de beaucoup d'autres, pourroient recevoir une nouvelle lumière des observations à faire sur les animaux, qui semblent s'éloigner le plus de notre manière d'être. Surpris par les phénomènes qu'ils nous présentent, on fouille avec avidité dans le sein de la nature; on y aperçoit des effets qui nous apprenent que la conformation la plus intime de certains animaux ne s'accordant pas avec nos anciennes idées, nous devons rejeter comme préjugés, des loix qu'on s'étoit trop pressé de généraliser. Ces effets, et la manière de les observer, qui semblent au premier coup d'oeil devoir nous rapprocher peu à peu de la cause, soutenir nos espérances, flatter

notre

us, by degrees, to investigate the cause, support our hopes, and flatter our vanity; are nevertheless meant to shew us that the marvellous works of nature far exceed what we know, or even are able to know of them. Convinced of that truth, and in order to improve my customary walks by the sea side to some useful purpose, I bestowed a particular attention, in the month of November 1771, upon the *Sea-Anemonies*, a most singular species of animals, on which naturalists have hitherto made less ample and accurate observations. Some of those writers have even confined themselves to mere conjectures. My first success in these discoveries soon turned these amusements of mine into a long study, much more laborious than that made in a library.

There

notre vanité, n'en paroissent pas moins destinés à nous faire voir que les merveilles de la nature sont bien au dessus de ce que nous connoissons, et sans doute de ce que nous pouvons connoître. Pénétré de ces vérités, et dans la vue de répandre une sorte d'intérêt dans les délassemens que j'ai coutume de prendre au bord de la mer, je jettai les yeux dès le mois de Novembre 1771, sur les *Anémones de Mer*, animaux singuliers sur lesquels les naturalistes n'avoient fait jusque là que des observations moins précises et moins étendues; souvent même s'en sont-ils tenus à de simples conjectures. Mes premiers succès changerent bientôt ces récréations en un travail long et plus pénible que ceux du cabinet.

There is great confusion in the descriptions which naturalists have given of these animals, and no less in the names bestowed upon, and the divisions or classes assigned to, them. Some have called them *Sea-Nettle* (*Urticæ Marinæ*), though these animals are not prickly, as some of the wandering *Nettles* are, upon which I have, for some time past, made observations which I intend to pursue. Other writers have called them *Sea-Anemonies*. This essay, together with the plates annexed, will satisfy the reader as to the name I have preferred. It would be needless to insist longer about names.

The *Sea-Anemonies*, found on the coast of the Havre, seem to me to constitute three different species. Those which I put in the first class (because in certain positions they resemble most the flower known by the name of Anemone) cling or adhere to rocks and stones, and are often found in the holes that
 chance

Il regne une grande confusion dans les descriptions que nous ont donné de ces animaux les naturalistes, dans les noms qu'ils leur ont imposés, et dans les divisions qu'on en a faites. Quelques uns les nomment Orties Marines (*Urticæ Marinæ*) quoiqu'ils ne piquent point comme quelques orties errantes, sur lesquelles j'ai fait depuis longtemps des observations que je me propose de continuer. D'autres leur ont donné le nom d'*Anémones de Mer*. Ce mémoire, et les figures qui l'accompagnent pourront justifier le choix que j'ai fait de ce nom. Il est inutile d'étendre plus loin cette nomenclature.

Les rivages du Havre offrent des Anémones de Mer, qui m'ont paru former trois especes. Celles que je regarde comme de la premiere (parce que dans certaines positions elles ressemblent plus parfaitement à la fleur qui porte le nom d'Anémone)
 s'attache

chance to be in them, and seem to like the surface of the water. The outward shape of the body of this animal, when it contracts itself, is much like a truncated cone [TAB. XVI. fig. 1.], with its basis fixed and strongly clinging to the rock. Its upper part is terminated with a hollow. This cone is often perpendicular to its basis; sometimes it lies in an oblique position to it, or the basis spreads itself irregularly; so that from a round, it alters to an elliptical shape. Sometimes it imitates pretty exactly the inclosing out-leaves of anemones, whilst the limbs of the animal are not unlike the shag, or inner part of these flowers, fig. 2. At other times, it assumes the shape expressed by fig. 3. Indeed, these animals alter their forms so often, that it would be difficult, perhaps even impossible, to describe them exactly. One part of their

s'attache aux rochers, aux cailloux, souvent même dans les trous qui s'y trouvent, et se plait à la surface de l'eau. La forme extérieure du corps de l'animal, lorsqu'il est retiré sur lui même approche beaucoup de celle d'un cône tronqué, (Pl. XVI. Fig. 1.) dont la base est appliquée et fortement attachée contre les rochers, et la partie supérieure se termine par une convexité. Souvent ce cône est perpendiculaire à la base; quelquefois il est à son égard dans une position oblique, la base s'élargit irrégulièrement, en sorte que de ronde elle devient elliptique. On la voit quelquefois imiter assez bien le manteau d'une anémone, tandis que les membres de l'animal paroissent en être la pluche, Fig. 2. d'autres fois il prend la forme de la figure 3. D'ailleurs ces animaux varient si souvent leur forme, qu'il seroit difficile, peut-être même impossible, de les bien décrire. Une partie de leur

B b b a

corps

their body or limbs swells, at times, very considerably, at the expence of the rest. The figures and the particular observations will supply what is wanting here. With regard to their colours, they vary amazingly. Every hue of purple, green, brown and violet is to be seen blended together. A great number of them are of one uniform colour; whilst others are spotted either symetrically, as in stripes, or in an irregular, but always pleasing, manner. Most of them have, round their basis, a blue or white streak, broader or narrower, which produces a sort of ring. When many of these animals are put together at the bottom of a flattish and wide vessel, the whole appears as a bed of Anemonies.

The Sea-Anemonies of the second species are pretty nearly shaped out as those of the first, but they are much larger. I have some by me, kept in
sea

corps ou de leurs membres s'augmente quelquefois considérablement aux dépens des autres. Les figures et le détail des observations suppléeront à ce qui manque ici. Quant à leurs couleurs elles varient étonnement; toutes les nuances de pourpre, de verd, de brun, de violet y sont employées et combinées. Un grand nombre sont partout d'une même couleur, d'autres ont des taches symétriquement rangées formant des rayes, ou même distribuées irrégulièrement, mais toujours d'une manière agréable. Presque toutes ont autour de leur base une bande bleüe ou blanche, plus ou moins étroite, qui y forme une bordure. Lorsqu'on en a beaucoup d'ouvertes dans le fond d'un vase plat et large, il semble d'un parterre d'anémones.

Les Anémones de Mer de la seconde espèce sont à peu près formées comme celles de la première, mais elles sont beaucoup plus grandes. J'en conserve dans l'eau de mer, dont les
membres

sea water, which are eighteen or twenty inches in circumference. Their cloak or outward skin is rough like shagreen, or full of little knobs. See Tab. XVI. fig. 10. and Tab. XVII. fig. 11. and 12. where they are shewn in the natural size. They remain in the sand, sticking to the loose stones in it, and stretch out their limbs to the top, in order to lay hold of their prey, as soon as it touches the superficies of the sand. The flower of poppies is said to be the plague and distress of painters, to represent exactly the variety and brilliancy of its colours; the same may be said of the Sea-Anemonies of this larger species. The purest white, carmine, and ultramarine, would hardly be bright enough to paint them properly. The limbs of some of them are of a moderate or dim colour, at the same time that the cloak is made up of the brightest colours.

The third species seems to deviate a little more from the second, than this from the first. Its body, not unlike for shape and colour to the stalk of a mush-

membres étendus forment une circonférence de 18 à 20 pouces; leur robe (la peau extérieure) est chagrinée ou remplie de boutons; voyez la Pl. XVI. Fig. 10. & XVII. Fig. 11. 12. où elles sont représentées de grandeur naturelle et moyenne. Elles restent dans le sable, attachées à des cailloux, et étendent leurs membres à la superficie pour saisir la proie qui les touche. S'il est vrai que les têtes de pavots fassent le désespoir des peintres par la variété et l'éclat de leurs couleurs, on pourroit en dire autant des Anémones de Mer de cette grande espèce; le beau blanc, le carmin, l'outremer, suffiroient à peine. Quelques unes ont les membres d'une couleur douce et éteinte, tandis que la robe offre des couleurs riches et fort hautes.

La troisième espèce semble s'écarter un peu plus de la seconde que celle-ci de la première; elle est aussi dans le sable, son corps

mushroom, is terminated in its lower part by a basis, which the animal fixes to the stones in the sand, whilst, by lengthening out its body, it affords means to the superior part, where the limbs and the mouth are placed, of spreading out and opening themselves at the surface of the sand. See fig. 4. This species has some slight variety in point of shape, and still more of colour. Some have their limbs of a bright white, or fine violet colour; others of an ivory white. Some are found of the same sort of yellow with the inside of melons. Some are greenish, or of a fine brown, with the middle white, which gives them a likeness to *Auricularas*. Others again have their limbs of a greyish tint, somewhat like the inside of a broken piece of silver; or alternately mixed with black and white in the manner of the quills of a porcupine.

I shall now proceed to the particular experiments and discoveries I have made hitherto upon the three species.

corps assez semblable pour la forme et pour la couleur au pédicule d'un champignon, est terminé à la partie inférieure par une base que l'animal attache aux cailloux dans le sable, tandis que le corps en s'allongeant permet à la partie supérieure, où sont les membres et la bouche de s'ouvrir à la superficie, Fig. 4. Cette espèce a quelques légères variétés de forme et encore plus de couleur; les unes ont les membres d'un beau blanc, ou d'un beau violet, d'autres couleur d'ivoire; il s'en trouve qui les ont couleur de chair de melon jaune; plusieurs sont verdâtres ou d'un beau brun avec un milieu blanc qui leur donne de la ressemblance avec la fleur qu'on nomme oreille d'ours; d'autres ont les membres gris d'argent cassé, ou mêlé alternativement de blanc et de noir, comme les piquans d'un porc épie.

Passons au détail des expériences et des découvertes que j'ai faites jusqu'ici sur ces trois espèces. En justifiant ce que

species. In justifying what I laid down at first, they will convince us more and more what little progress has been made in the knowledge or history of *reproductions*; how far the resources and powers of nature may extend; the uncertainty of our conjectures both upon what really constitutes the animal, and the exact moment that it passes from life to death. What an extensive field does the study of animal œconomy, that most beautiful and useful part of physic, offer still to our investigation! At the same time that it encreases our knowledge in that branch, which is looked upon as the basis of the art of healing, it may help us to find out whether our sea-flowers are destructive to crabs and other *crustaceous* fish, to mussels, &c. and whether they themselves might not prove a palatable and useful food. These researches might also afford proofs that the structure of those animals which are looked upon as little deserving of our attention, are still, some

que j'ai établi au commencement, elles pourroient faire entrevoir combien nous sommes peu avancés dans l'histoire des reproductions; quelles peuvent être les ressources et les forces de la nature; l'incertitude de nos conjectures sur ce qui constitue l'animal, et sur le moment précis où il passe de l'état de vie à l'état de mort. Quel vaste champ nous offre encore à cultiver l'étude de l'œconomie animale! cet objet le plus beau et le plus utile que la physique nous présente, est propre en même temps à augmenter la somme de nos connoissances dans cette partie qu'on regarde avec raison comme la base de l'art de guérir; à déterminer si ces animaux détruisent les cancreux et autres crustacés, les moules, &c. ou s'ils ne pourroient pas devenir eux memes un mets recherché et délicat; à nous convaincre que la structure des animaux qu'on juge peu dignes d'attention,

some by their complicate make, and others by their very simplicity, more incomprehensible than larger and better-known animals. It may in short keep up our admiration, by observing how the omnipotence of the Creator shines forth even in creatures, which, like these, seem produced to be trampled upon, or at best to be perceived by chance.

What first offered itself to my observations, is what distinguishes these animals from plants, I mean progressive motion, by the help of which they can shift their spot; the other determinate motions, by which they are enabled to lay hold of their préy; the means they make use of to defend themselves; their deglutition, digestion, evacuations, and lastly the propagation of their species, &c. What little I have had an opportunity to see of those functions, appears to me sufficient to place these creatures in the class of spontaneous animals, rather than in the dark indeterminate list of *zoophytes*.

In

d'attention, offre dans les uns par sa complication, dans les autres par sa simplicité, quelque chose de plus incompréhensible que celle des animaux plus grands et plus connus; enfin à soutenir notre admiration en nous faisant observer combien la grandeur de Dieu éclate jusque dans les créatures qui semblent comme celles-ci destinées à être foulées aux pieds, ou à n'être aperçues que par hazard.

Ce que j'ai du d'abord observer, c'est ce qui distingue ces animaux des plantes, comme le mouvement progressif à l'aide duquel ils changent de place, les autres mouvemens déterminés, par lesquels ils saisissent la proie, les moyens qu'ils emploient pour se défendre, la déglutition, la digestion, la déjection, la propagation de l'espece, &c. le peu que j'en ai vu m'a paru suffisant pour leur assigner une place dans le regne animal, et pour les tirer du genre obscur et indéterminé des *zoophytes*.

Au

In May 1772, I clipped all the limbs of a purple Anemone of the first species. Soon after, these limbs began to bud out again. The 30th of July, they were clipped a second time, and grew again in less than a month. Having cut them a third time, they had a third shooting out. The same experiment upon a green Anemone had the like success. It seems these reproductions might extend as far, or be as often repeated as patience and curiosity would admit. Several experiments have convinced me that one single limb of these Anemonies being cut off, retains a power to fasten itself to any small body that is brought near it, either by its end, or by the side towards the end, but not by that part where the clipping was made. This induces me to think that the effect is produced by suction rather than by any glutinous matter, which one might suppose oozes out at the pores. This limb, after being cut off, has also a power to stretch or contract itself alternately.

July

Au mois de Mai 1772, j'ai coupé tous les membres à une anémone pourpre de la première espèce. En peu de temps ces membres repoussèrent. Le 30 Juillet ils furent coupés de nouveau, et se reproduisirent en moins d'un mois. Ayant été coupés une troisième fois ils ont encore repoussé. Une anémone verte a donné le même résultat. Il paroît que ces reproductions iroient aussi loin qu'on auroit la curiosité ou la patience de les pousser. Plusieurs expériences m'ont appris que jusqu'à un seul membre retranché de ces animaux, conserve pendant plusieurs jours la faculté de s'attacher aux corps qu'on lui présente, soit par le bout, ou par le côté vers le bout, et non par la partie coupée; ce qui m'a porté à croire que cet effet pouvoit être produit par la succion plutôt que par la glu, qu'on soupçonneroit exuder des pores. Ce membre coupé se contracte et se dilate aussi alternativement.

July the 12th I cut one of these purple Anemones through the body, rather nearer the basis. This part remained adhering to the side of the vessel in which it was; and for several days, made various motions. At last, it got loose, and then fastened in another place. The 27th it began again to move about, till the end of August, when it became as it were lifeless, grew very flabby, and had often an offensive smell. I concluded it to be dead; but, as it did not lose its shape, I resolved to keep it, and to shift it every day into some fresh sea-water. From time to time, I thought it had some sort of motion, and in the beginning of November these motions became more perceptible. It shifted its position, when contrary to its natural state. November the 28th, this *stump* climbed up to the top of the vessel. I then began to perceive some new limbs growing out. January 13,

14,

Le 12 Juillet je coupai par la moitié du corps et même plus proche de la base une anémone pourpre. Cette base resta attachée aux parois du vase, et fit pendant plusieurs jours différentes marches, après lesquelles elle se détacha, et s'attacha de nouveau. Le 27 elle recommença ses courses jusqu'à la fin d'Août, où elle cessa de donner sensiblement des signes de vie, devint d'une mollesse extrême, et répandit même à plusieurs reprises une mauvaise odeur: je la jugeois morte; cependant comme elle ne se déformoit point, je pris la résolution de la garder, et de lui procurer tous les jours de nouvelle eau de mer. Je crus appercevoir de loin à loin quelques mouvemens. Au commencement de Novembre, ses mouvemens devinrent plus sensibles, et jusqu'à se retourner, quand elle étoit dans une position contraire à celle qui lui est naturelle. Le 28 Novembre elle grimpait au haut du vase, alors je crus appercevoir des membres renaissans. Les 13, 14, 15, 16 Janvier, elle a acquis de

un nouveau

14, and 15, 1773, it again walked about; and on the 16th, seeing these growing new limbs, I offered them some bits of muscles, which however were neither eat, nor even laid hold of. That same day, after several motions in various directions, it loosened its adhesion, and remained motionless and flabby, but without any bad smell, till the beginning of February, when it appeared adhering, but weakly, to the bottom of the vase. The 16th, after several motions, it climbed up to the top, where it remained till the 11th of March, and then loosened its hold. These alternate stations and motions lasted till the 8th of April, without my seeing any plain and full re-production. However the animal continues to grow stronger and thicker. I own I was very wrong to throw away, a few days after the operation, the upper part that had been cut off. But I did not foresee what would happen.

November

nouveau. Le 16 appercevant des apparences de membres je leur presentai de petits morceaux de moule, qu'elle n'a ni mangés ni retenus. Ce meme jour, après plusieurs mouvemens en divers sens, elle s'est détachée et a resté ainsi molle et immobile jusqu'au commencement de Fevrier, sans pourtant porter de mauvaise odeur; ensuit celle a paru foiblement attachée au fond du vase; elle a monté au haut le 16, après plusieurs mouvemens, où elle a resté jusqu'au 11 Mars, qu'elle se détacha. Ces alternatives et ces mouvemens ont duré jusqu'aujourd'hui 8 Avril, sans que je voye paroître de reproduction réelle; cependant elle continue de se fortifier et devient de plus en plus épaisse. Je jetai sans doute fort mal à propos dès les premiers jours la partie retranchée; pouvois-je prévoir ce qui devoit arriver.

November the 9th, 1772, I clipped a brown Anemone through the body. The basis, together with that part of the stump which was left to it, shrunk up, as figure 5. shews, and remained motionless where it was at first, till January 13, when it shifted its place. The 15th, I very distinctly perceived two rows of limbs growing out of the part where the section was made, fig. 6. and the animal moved along. The next day I offered it bits of muscles, which it laid hold of and ate. These growing limbs were, at first, of a sullied white, they grew browner and browner every day, and ate at present of the same colour with the coat of the animal. They are pretty near as large as they were before the operation; but I have not perceived, as yet, some of those small fine blue knobs, that are to be seen round the rim or upper ~~margin~~ of the coat, as may be seen in figure 2. As

Le 9 Novembre je coupai par la moitié du corps une anémone brune; la base avec la partie du tronc qui lui restoit se retira sur elle même, comme la représente la figure 5; elle resta immobile au lieu où elle étoit, attachée, jusqu'au 13 Janvier qu'elle marcha. Le 15 j'aperçus très-distinctement deux rangs de membres, renaissans de la section, même, figure 6, et l'animal marcha. Le lendemain je lui présentai des morceaux de muscles, il les saisit et les mangea. D'abord ces membres renaissans étoient d'un blanc sale; mais peu à peu ils ont bruni et approchent maintenant de la couleur de la robe de l'animal. Ils sont très-peu plus grands qu'ils n'ayent la même grandeur qu'ils avoient auparavant; je n'ai cependant point encore vu paraître certains petits globes d'un beau bleu engagés dans le trou du tronc au-dessus du supérieur de la robe, comme on peut voir par la figure, concernant à la partie détachée, représentée par

to the part cut off, seen in figure 7, which consists of about half the body, and wherein the limbs and mouth are placed, I offered it, after the operation was performed, that brown part of a muscle, by the help of which it moves along, and whence the beard spreads out. This bit, which is not easily digested by Sea-Anemonies, was directly snapped up by the limbs. They drew it to the mouth, which lengthened itself out to catch it, and swallowed it down. But, as the body was wanting to receive it, the bit came out at the opposite end, just as a man's head, being cut off, would let out, at the neck, the bit taken in at the mouth. I offered it a second time, and the animal swallowed it again; but threw it up at the mouth the next day. I still keep that part of the Anemone, which daily grows stronger and stronger, and which appears to me to suck in the bits of muscles I offer it. The limbs lay hold of them, and the mouth takes them in, either whole or in part, and throws them

up

par la fig. 7. qui comprend environ la moitié du corps, où se trouvent les membres et la bouche, je lui présentai peu après l'opération un morceau de moule où étoit ce membre brun qui lui sert à se traîner, et où est attaché la filière, morceau que les anémones de mer ont peine à digérer; les membres le saisi-
rent, l'approchèrent de la bouche, la bouche même s'allongea pour le recevoir, et il fut avalé: mais comme il n'y avoit point de corps entier pour le recevoir, il sortit par l'autre extrémité, comme une tête tranchée qui avaleroit pour le rendre par le col. Je la lui présentai de nouveau, elle l'avalait une seconde fois et la rendit par la bouche le lendemain. Je garde encore cette partie qui se fortifie chaque jour et on voit paroitre les morceaux de moule que je lui présente; les membres les embrassent, la bouche les reçoit en tout ou en partie, et me les rend fortifiés, sont ent

up a good deal altered ; and pretty often these bits go through as they did the first time. Some persons, who were eye-witnesses of these particulars, were of opinion that, from the remains of organization and habit, this part of the animal still endeavoured to gratify a natural want, though no longer subsisting ; but I am inclined to think that it still exists. In my opinion the part is nourished by means of suckers, of which I suspect it to be full, both inwardly and outwardly. I am in expectation to have my conjecture confirmed, by experience, and by it to be enabled to convince other people. The microscope seems to have already corroborated my notion upon this subject. When the limbs of these Anemonies, especially those of the *second* species, are touched, the person's fingers are felt to adhere and strongly to stick to the limbs. I have therefore let both these, and several other species of these animals, fasten several times upon the fingers of one of my hands, in order to see whether any glutinous matter should remain upon them ; but I never

souvent ils passent d'outre en outre comme la première fois. Ceux qui en ont été témoins pensoient que, par un reste d'organisation et d'habitude, cette partie de l'animal cherchoit encore à satisfaire un besoin qui n'existoit plus ; pour moi je suis porté à croire que ce besoin existe, que la partie peut se nourrir par des suçoirs dont je soupçonne qu'elle est remplie intérieurement et extérieurement, et j'attens de l'expérience ce qui me manque pour le prouver aux autres. Le microscope m'a déjà paru fortifier mon sentiment ; lorsqu'on touche les membres des anémones entr'autres de la seconde espèce, on sent ses doigts fortement attachés à ces membres. Je les ai donc laissés s'attacher à plusieurs reprises et sur plusieurs anémones pour voir s'il ne resteroit pas à mes doigts quelque sorte de glu, et je n'y en ai jamais

never perceived any; and by applying the fingers of that hand to the other, I perceived no adhesion.

I have, for some days past, clipped several Anemonies of the first species, diametrically and perpendicularly to the basis. They have stood the operation extremely well. Time will teach us what the result of these operations will be.

The 5th of February last, I placed a glass vessel, exposed to the morning frost, with two Anemonies, the one brown the other purple, which stuck to the sides of it towards the top. The surface of the water froze, and remained frozen the whole day: in the night-time, the thickness of the ice increased to such a degree, that there remained but a little water not frozen in the bottom of the glass. The Anemonies were thus inclosed in a pretty strong piece of ice, except the basis, which remained adhering to the bottom. The glass was next day placed near the fire, in order to melt the ice by degrees, and
when

jamais apperçu : j'ai porté ces doigts sur mon autre main, et il ne s'en est suivi aucune adhérence.

J'ai coupé depuis plusieurs jours des anémones de la première espèce diamétralement et perpendiculairement à leur base, elles ont soutenu l'opération fort bien; le temps nous en dévoilera les résultats.

Le 5 Février dernier au matin j'exposai à la gelée un vase de verre contenant deux anémones, l'une brune, l'autre pourpre, attachées aux parois vers le haut du vase; la surface de l'eau se glaça, et resta en cet état toute la journée; pendant la nuit l'épaisseur de la glace augmenta tellement qu'il ne restoit qu'un peu d'eau dans le fond, de sorte que de tous côtés excepté la base adhérente au verre, mes anémones étoient enfermées dans un fort glaçon; le lendemain j'e mis le verre devant le feu pour faire fondre peu à peu la glace. Quand elle

when it began to float, I saw plainly the Anemonies were quite detached from the sides, because their body turned round loose, with the ice, though their basis had remained exactly in the same situation they were in before. Mr. *Reaumur's* thermometer being in my room, at two degrees above the freezing point, or 0, these animals gave some sign of life; and the 8th, the thermometer being at $3\frac{1}{2}$ they opened; while the Anemonies, which were in other glasses, and had not been exposed to a greater cold than that of one degree above frost, did not open yet; and I generally observed they only opened at 5° of the thermometer. May it in consequence be supposed that those Anemonies, which had been frozen in the ice, were less affected with a cold at $3\frac{1}{2}$ than the others?

March

elle devint flotante, j'eus lieu d'apercevoir que les anemones avoient cessé d'être attachées aux parois du vase par leur base, parcequ'elles tournoient avec la glace, quoique leur base eut absolument demeuré dans le même état où elles étoient auparavant. Le thermometre de Mr. de Reaumur étant dans mon cabinet à deux degrés de dilatation, ces animaux donnerent quelques signes de vie, et le huit le thermometre étant à $3\frac{1}{2}$ s'ouvrirent tandis que celles des autres vases qui n'avoient point éprouvé de froid plus grand qu'un degré de dilatation ne s'ouvrirent pas encore. Car j'ai remarqué qu'elles ne s'ouvrent, à parler généralement, que lorsque le thermometre parvient à cinq degrés. Celles qui ont été enfermées dans la glace auroient-elles par comparaison trouvé la temperature de trois degrés et demi plus douce, que ne la trouvoient les autres ?

March the 19th, I set upon the fire a glass vessel, with the two animals which I had made use of for the foregoing experiments. A thermometer put into the same glass was at 8° ; when it came to 15 and 17, the Anemonies opened themselves a little; but, at 28 and 30, they appeared to be in pain; at 32 they closed themselves up; at 37 one of the two fell off from the sides; upon which I poured in some fresh water, and took the glass from the fire. Soon after they opened, and one of them produced two young ones the next day. In another experiment, the same Anemone which dropped off at 37 degrees, detached itself at 26, and closed up at 28. In a third experiment I shifted, at once, one of these Anemonies from 8° to 40° , and left it to remain in that heat for five minutes; it appeared to be in pain for several days after, its coat was even affected on one side, but,

at

Le 19 Mars je plaçai sur un feu doux un vase de verre, contenant les deux anémones qui m'avoient servi aux expériences précédentes, avec un thermomètre plongé dans le même vase; il étoit à 8 degrés de dilatation, lorsqu'il est parvenu à 15 et 17; les anémones se font un peu ouvertes; mais à 28 et 30, elles ont paru souffrir; à 32 elles se font fermées; à 37 une des deux s'est détachée des parois; j'ai alors promptement remis de nouvelle eau, et retiré le vase du feu, peu après elles se font ouvertes et une a donné deux petits le lendemain. Dans une autre expérience, la même anémone qui s'étoit détachée à 37 degrés, s'est détachée à 26, et s'est fermée à 28. Dans une troisième expérience, j'ai fait passer subitement une de ces anémones de 8 degrés de dilatation à 40; je l'y ai laissée pendant cinq minutes; elle a paru souffrir pendant plusieurs jours, sa robe a même été entamée par un côté; mais enfin

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at last it recovered its strength, clung to the sides, climbed up to the top of the glaſs; and all was well again. I have ſhifted another Anemone at once from 8° to 40° : but, inſtead of leaving it only five minutes, I went on increaſing the heat to 46° , in order to obſerve the effect. The coat appeared to me to ſwell out a little; at 50° it ſtill encreaſed, and the animal died in conſequence.

I put ſeveral Anemonies, ſome in, and ſome without, water, under the recipient of an air-pump; and got the quickſilver of the barometer incloſed in it, to come down within an inch of the level. The animals did not ſeem to perceive the effects of a *vacuum*, or an air ſo much rarefied. They remained open, or in whatever ſtate they happened to be when put in, and did not ſwell out. When the air was let in again, and they were not in the water, I perceived no ſhrinking at all in any of them.

THESE

elle a repris vigueur, ſ'eſt attachée, et a gagné le haut du vaſe; il n'y paroît plus. Du même point de 8 degrés j'ai fait paſſer ſubitement une autre anemone à 40 , et au lieu de ne l'y laiſſer que cinq minutes, j'ai obſervé ce qui ſe paſſeroit en continuant de chauffer; à 46 degrés la robe m'a paru ſe bourſoufler un peu; à 50 degrés elle l'étoit encore plus, de ſorte que l'animal en eſt mort.

J'ai mis dans l'eau, et ſans eau, ſous le récipient d'une machine pneumatique pluſieurs anemonies, j'ai abaiffé le mercure du barometre qui y étoit renfermé, juſqu'à un pouce de ſon niveau. Elles n'ont pas paru ſ'appercevoir de l'effet du vuide, ou d'un air ſi fort rarefié: elles ont reſté ouvertes, ou comme elles étoient, ne ſe ſont point gonflées, et lorsſque j'ai rendu l'air au récipient, et qu'elles n'étoient point dans l'eau je n'ai apperçu en elles aucun affaiſſement.

These animals can live a whole twelvemonth, and perhaps much longer, without any other food but what they chance to find diffeminated in the sea-water. They do not want many motions to procure their food, besides stretching out their limbs, to receive such as comes within their reach; and they remain surrounded with muscles, &c. without laying hold of any of them. I have given Anemonies some of these muscles alive, but with their shells closed, and about six lines in length. They were swallowed in that state; and 40, 50, and 60 hours after, the shells were thrown up at the mouth empty and perfectly cleared, even from the small tendons which connect the fish to its shells. The Anemonies swallow and digest small fish, and bits of larger fish, or of raw meat, when offered to them. When they cannot digest some of the food, they throw it up at the mouth, either whole or partly dissolved into

Il paroît que ces animaux peuvent vivre pendant une année, et peut-être beaucoup plus, sans autre nourriture que celle qu'ils trouvent difféminée dans l'eau de la mer; qu'ils ne font pas beaucoup d'autres mouvemens pour s'en procurer d'autre que celui d'étendre leurs membres pour recevoir celle qui vient d'elle même y toucher: car ils restent environnés de moules, &c. sans s'en emparer. Je leur en ai donné de 6 lignes de longueur vivantes et fermées, les anémones les ont avalées en cet état, et 40, 50, ou 60 heures après, elles m'ont rendu par la bouche les deux coquilles vuides et parfaitement nettoyées, même des petits tendons qui attachent le poisson aux deux coquilles. Elles avalent et digèrent des petits poissons, et des morceaux de poisson ou de viande fraîche que je leur présente. Lorsqu'elles ne peuvent digérer quelque chose, elles le rendent

into a viscous liquor, which may in some measure be looked upon as their excrements. They void by the same way, and in the same manner, various parts of a white and brown substance, and small bodies are thrown out at the extremity of the limbs, and more sensibly in the second species. There also ouzes out of their body a sort of excrementitious matter, which by coagulating produces round them a sort of girdle.

These animals are known to be *viviparous*. Several of them have brought forth, even in my hand, 8, 10, and 12 young ones. Some, though almost imperceptible, as in fig. 8, have nevertheless the power of clinging, and are endowed with two rows of limbs, which they open immediately upon their birth, in order to catch their prey, which they swallow afterwards. I have kept some for ten months. They have appeared to me not to have increased in their bulk, more than
twice

dent par la bouche ou en entier ou réduit en partie en une espece de glaire qu'on peut regarder comme leurs excréments, quo qu'on leur voit rendre aussi par la même voye différentes parties de matiere blanche ou brune, et de petits corps par le bout de leurs membres, ce qui est plus sensible dans la seconde espece. Il sort aussi de leur corps une sorte d'humeur excrementitieuse, qui forme tout autour en se coagulant comme une espece de ceinture.

Ou sait que ces animaux sont vivipares; plusieurs m'ont donné, même dans ma main 8, 10 et 12 petits, les uns presque imperceptibles, figure 8, n'en ont pas moins la faculté de s'attacher, et ont un ou deux rangs de membres qu'ils ouvrent aussitôt après leur naissance pour saisir la proie qu'ils avalent ensuite; j'en ai gardé pendant dix mois; ils ne m'ont paru augmenter en grosseur que du double de leur diamètre. Il est
vrai

twice the diameter of their size. Indeed I fed them sparingly; the others are grown as big as half a green pea. Fig. 9.

The Sea-Anemonies have a progressive motion; which, though slow, is performed in every direction, with a degree of facility, and effected by means of muscles, which cross one another at right angles. I cannot as yet display the mechanism of these muscles, because the last mutilations I was obliged to make discompose the conjectures that some men of learning have published upon this subject; and I have not sufficiently fixed my thoughts in consequence of my observations.

These animals, being put into fresh water, die very soon after, and whatever their colour be when they are put in, they soon turn pale, and remain pretty near in the same position that they were in. Soon after, their coat becomes so flabby, that it can
very

vrai que je leur ai donné peu de nourriture; les autres sont gros comme la moitié d'un pois verd, Fig. 9.

Les anémones de mer ont un mouvement progressif, à la vérité assez lent, mais qui s'exécute pourtant en tous sens avec une sorte de facilité. Il dépend de muscles qui se croisent à angles droits; mais je ne puis en développer ici le mécanisme, parceque les dernières mutilations que j'ai faites déconcertent les conjectures que quelques savans avoient publiées sur ce point, et qu'en conséquence je n'ai pu encore fixer mes idées à ce sujet.

J'ai mis des anémones de mer dans de l'eau douce, elles y ont péri presque aussitôt, on les voit pâlir un peu, de quelque couleur qu'elles soient et demeurer à peu près dans la même position qu'elles étoient au moment qu'on les y a plongées. Peu de temps après, leur robe devient si facile à entamer, qu'un pei-

very easily be stripped off in shreds. For this reason, when I boiled them, I always made use of seawater.

I have not been able hitherto to perceive any eyes in these creatures; and yet they seem to be affected by the light. I have had opportunities to satisfy myself on this subject at every hour of the night. For by holding up a lighted candle over glasses, which contained a great number of them, but at such a distance as not to communicate any heat, I could see them, either by degrees, or even suddenly, close up, and not spread out again 'till a while after the light was removed. The same effect was produced upon repeated trials. This, however, admits of some exceptions, viz. when they had fed plentifully, they were slower in shutting themselves up, and even sometimes did not close up at all. The brighter the light let in upon
them

pinceau l'enleve aisément par lambeaux; c'est pourquoi quand j'en ai fait cuire, j'ai employé de l'eau de mer.

Jusqu'ici les anémones ne m'ont point paru avoir d'yeux, elles sont pourtant affectées par la lumière. J'ai eu occasion de le remarquer à toute heure de la nuit; car en tenant au dessus des vases, qui en contenoient un grand nombre, une bougie allumée de laquelle elles ne pouvoient ressentir aucune chaleur, je les voyois peu à peu quelquefois même subitement se fermer, et elles ne s'ouvroient de nouveau que lorsque je faisois disparaître la lumière pour quelque temps, après lequel si je la leur présentois, elles se fermoient comme la première fois. Ceci a quelques exceptions qui ne détruisent point la loi générale; par exemple, lorsqu'elles ont beaucoup mangé elles se ferment moins vite et même point du tout. Plus la lumière est vive en proportion de celle dont elles jouissoient

them is, in proportion to the degree of that which they were in before, the more affected do they appear to be by it. Is then the texture of the body of those creatures such, as not, indeed, to receive the impressions of objects, with the same degree of perfection as our eyes do ; but, to afford a general organ affected by light, as the bodies of other animals are by feeling ?

Might not these animals become a fresh supply for our tables ; and thereby make amends for the consumption of muscles, on which they feed ? In order to try the experiment, I gave some Anemonies, boiled in plain water, to a cat, who devoured readily twenty of them, several of which were of the third species, without any bad effect, and seemed wishful to have more. This induced me to make a further trial, and to get some of them colloped as we do oysters
in

auparavant, plus elles y paroissent sensibles. Le corps de ces animaux seroit-il donc, je ne dis pas un organe capable de recevoir l'impression des images, avec les conditions nécessaires à une sensation aussi parfaite que la procure notre oeil, mais une espece d'organe général sensible au contact de la lumiere comme le toucher des autres animaux l'est à celui des autres corps ?

Ces animaux ne pourroient-ils point devenir une nouvelle ressource pour nos tables, et réparer ainsi la perte des moules dont ils se nourrissent ? pour m'en assurer, j'en fis donner, de bouillis, sans aucun assaisonnement, à un chat, qui en mangea avec avidité vingt, dont plusieurs étoient de la troisième espece ; il n'en parut ressentir aucune incommodité, on jugea même à son air qu'il en auroit mangé d'avantage. Cela me fit desirer d'avoir sur cet objet essentiel une plus grande certitude, j'en fis préparer comme on fait les huîtres dans ces grands peignes, qu'on nomme coquilles de pélerin, et les ai mangés

in shells, I swallowed them up myself. Perhaps they may prove still more palatable, dressed in some other manner. However, they may be eat safely.

The second species of Sea-Anemonies keeps itself hid more than the first; it is not to be come at but in neap-tides, when the sea recedes farthest, and cannot be so easily observed. With great difficulty are these Anemonies loosened from the stones they adhere to; part of the basis is often left behind, and they are not easily preserved at home. Seeing however some of the individuals that voided muscle-shells whole, and still joined together, but empty, I found out the way to feed them. Crab-shells, about the size of a hen's egg, are likewise discharged whole; and upon offering them some live-ones, I found that they swallowed them down, and voided the remains sucked dry in about twenty hours. I cut open some of this second species, which I could not loosen from the stones, and among them I found
one

mangés; peut-être une autre préparation leur conviendrait-elle mieux, toujours est-il certain qu'on en peut manger.

La seconde espece d'anemones de mer plus cachée que la premiere, et qu'on ne trouve que dans les grandes marées, où la mer se retire plus loin, est aussi plus difficile à observer; on a peine à la détacher des cailloux sans y laisser une partie de sa base, et à la conserver chez soi. C'est en observant quelques individus de cette espece et en appercevant qu'ils rendoient des coquilles de moules entieres, jointes ensemble et vuides, que j'ai appris de quelle maniere je devois nourrir mes anemones. Je leur ai vu même rendre des cancrs entiers grands comme la circonference d'un oeuf de poule, je leur en présentai de vivans qu'elles avalerent et qu'elles m'ont rendus morts, et sucés environ vingt heures après. J'en ai ouvert que je ne pouvois détacher des cailloux, parmi lesquelles j'en
trouvai

one that had swallowed an Anemone of the *third* species; but this had received no harm. For, having put it into some sea-water, it opened and spread as usual. I have offered them several of the same species, which they swallowed down; but threw up again alive within eight, ten, or twelve hours, or even later. Is then a live Anemone an undigestable body for another?

On the 21st of June, 1772, having *nicked* the instant, that an individual of the third species was stretching out, as expressed in Fig. 13, I snipped off at once with sharp scissars, the whole upper part where the limbs and mouth are placed. It was with great satisfaction that, eight days after, I perceived new limbs growing out, as in Fig. 15. The third of July, the animal began to eat some bits of muscles; and towards the middle of the month, the upper part was so completely formed, that it might easily

trouvai une qui avoit avalé une anemone de la troisieme espece qui n'en avoit souffert aucune altération: car l'ayant mise dans l'eau de mer, elle s'ouvrit comme à l'ordinaire. Je leur en ai présenté plusieurs, elles les ont rejetées en vie au bout de huit dix ou douze heures, et même plus tard. Une anemone en vie seroit-elle donc un corps indigeste pour une autre anemone?

Le 21 Juin ayant saisi l'instant où une des anemones de la troisieme espece étoit allongée, comme dans la figure 13, j'en retranchai subitement, avec de bons ciseaux, toute la partie supérieure où sont les membres et la bouche. Avec quelle satisfaction n'ai-je pas vu au bout de huit jours de nouveaux membres renaissans figure 15. Le trois Juillet l'anemone commença à manger des morceaux de moule, et vers la moitié du même mois, la partie supérieure étoit si bien reproduite,

easily have been mistaken for one of its unclipped neighbours, had there been many in the glass. It is neither the row of the central or inner limbs, nor the most outward, which first bud out, but the intermediate ones. The part, which had been clipped off, gave signs of sensibility to the 17th of July, contracting and dilating itself, in the same manner as a whole Anemone; but it was much smaller than before the operation. This experiment has been repeated by clipping, on the 11th of July, the whole upper part and one third of the body of another Anemone. New limbs began to shoot out the 21st. There were two rows of them the 25th; and on the 3d of August, four very distinct and well shaped, which caught and kept fast the food that was offered the animal. The mouth itself was sufficiently well formed to take in several times bits of muscles.

qu'on auroit aisément confondu l'anémone avec ses voisines, si elles eussent été en grande quantité dans le même vase. Ce ne sont ni les rangs des membres les plus voisins du centre, ni ceux qui en sont les plus éloignés qui paroissent les premiers dans la reproduction, mais les intermédiaires. La partie qui avoit été retranchée a donné jusqu'au dixsept Juillet des marques de sensibilité, se contractant et se dilatant de la même manière que le fait l'anémone: mais elle étoit beaucoup plus petite qu'avant d'être retranchée. Cette expérience singulière a été répétée en coupant le onze Juillet toute la partie supérieure et un tiers de la longueur du corps d'une autre anémone. Il reparut de nouveaux membres le vingt-un; le vingt-cinq elle en avoit deux rangs, le trois Aoust quatre bien formés et qui retenoient les corps qu'on lui présentoit, lorsqu'ils étoient propres à la nourriture: la bouche même commençoit à être assez bien formée pour que l'animal ait mangé plusieurs fois

de

muscles. On the 11th, I perceived in the limbs the faint alternate marks of ivory white and black, and soon after, there scarcely appeared any sign of the operation having been performed.

Being induced to try further experiments, on the 7th of August, I clipped an Anemone across the body. Like the others, it moved or wriggled a little at first: but, I did not perceive any new limbs growing till towards the end of the month. During that time, it continued in such a state as gave but little hopes of its doing well again. Two rows of limbs appeared at last, and the insect recovered its strength. There was on the 9th of September a third row of limbs, and the mouth appeared to be shaped out and formed; yet the Anemone neither ate nor kept the bits of muscles I gave it. A fourth row was growing out on the 19th, which gathered strength by degrees; so that on the

des morceaux de moule. Le onze j'aperçus dans les membres les petites alternatives de blanc d'ivoire, et de noir; et peu après il étoit difficile de s'appercevoir qu'elle eut souffert quelque alteration.

Tenté de pousser la chose plus loin, le sept Août je coupai par la moitié du corps une anémone. Elle se donna d'abord comme les autres quelques mouvemens, mais ce ne fut que vers la fin du mois que j'observai de nouveaux membres; pendant cet intervalle elle fut dans un état qui laissoit peu d'espoir, enfin deux rangs de membres parurent et l'animal reprit vigueur. Le neuf Septembre il y avoit un troisième rang de membres, et la bouche paroissoit formée; l'anémone cependant ne mangeoit ni ne retenoit les morceaux de moule qui lui étoient présentés: le dix-neuf on aperçut un quatrième rang de membres, qui peu à peu se fortifia, de manière que le trois

the 3d of October it began to eat, and in a short time became a compleat animal. On September the 22d, the upper part appeared to me to be withering away. But we shall soon see how much I have reason to think I was mistaken.

I cut another Anemone, of the same species, across the middle of the body, in such a manner as that the two parts were only left hanging together by one fourth part of the diameter. My design was, to try whether nature would produce limbs on the edge of the lower half-part, in the same manner as when the body is cut quite asunder; or, whether the wound, though very deep, would heal up again. Nature was not wanting to itself; for, notwithstanding the largeness of the incision, the two severed parts were joined up together, and in a few days the wound was
healed

Octobre l'anemone mangea et devint en peu de temps un animal complet. Le partie supérieure me parut périr le vingt deux Septembre; mais on verra combien j'ai lieu de croire que je me suis trompé.

J'ai encore coupé par la moitié du corps une anemone de cette espece, de maniere que les deux parties ne tenoient plus l'une à l'autre que par un quart du diametre du corps de l'animal. J'avois envie de voir si la Nature ne feroit pas naître des membres au coté coupé de la partie inférieure, comme lorsque la supérieure en est totalement séparée, ou si la playe quoique fort grande se consolideroit. La Nature ne s'est point trompée; malgré la grandeur de l'incision les deux parties se sont rapprochées, et au bout de quelques jours la
playe

healed up. The animal did not even seem to have suffered so much as one might have been apt to expect.

I have been witness to a fact remarkable enough to be inserted here. Having sliced a bit of fish, I offered one end of it to an Anemone, whose limbs are of the melon-yellow colour, and the other end to a grey Anemone, whose superior part had been re-produced after it had been cut off. The two insects, which were adhering pretty near one another at the bottom of the glass, directly seized their prey with their arms; but the yellow one happened to lay hold of the larger share of the slice. Each swallowed on, by the respective ends, till at last each other's mouth came within contact. The grey one seemed, at first, to get the better; but the other soon recovered her share, lost it again, and again recovered it. These alternate victories lasted about three hours; and

playe étoit consolidée. L'animal n'en a pas même paru souffrir autant qu'on l'auroit imaginé.

J'ai été témoin d'un fait assez singulier pour qu'il trouve place ici. Ayant coupé un morceau de poisson en forme d'aiguillette, je le présentai par un bout à une anémone, dont les membres sont couleur de chair de melon, et de l'autre bout à une anémone grise, dont la partie supérieure s'étoit reproduite après avoir été coupée. Mes deux anémones attachées au fond du vase assez près l'une de l'autre saisirent la proie avec leurs membres; mais l'anémone jaune eut lieu d'en embrasser une plus grande partie. L'une et l'autre avalèrent le bout qu'elles tenoient et continuèrent d'avalier jusqu'à se trouver bouche à bouche. La grise me parut avoir l'avantage; peu après il lui fut disputé, elle le recouvra, puis le laissa échapper, ensuite le reprit. Ces alternatives ayant duré pendant trois heures, je

and there was a time, during which the yellow Anemone was near being worsted; till at last, the grey one losing hold of her end of the slice, the other carried off the prize. Yet, as she sucked in but slowly, the grey one ventured with her mouth upon a last tug at her end, still in sight, which she had slipped; but this fresh effort proved fruitless: the yellow champion gave a last pull, and swallowed down the whole. During this whole strife, the two parties did not seem to be animated by any other passion than that of snatching the slice of fish from one another; and though the two animals continued afterwards to remain neighbours, they lived very quiet and peaceable together.

These animals are sometimes very voracious. Could it be believed that the same creature, that can continue in pretty good plight for a whole twelvemonth, and perhaps longer, without taking in any other food, besides what
may

vis un moment où les affaires de la jaune paroïssent aller assez mal, lorsqu'enfin la victoire se déclara pour elle: la grise lâcha le bout. Comme la jaune n'avaloit pas très vite, la grise eut le temps de revenir à la charge; elle saisit de nouveau par la bouche le bout, qu'elle venoit d'abandonner, mais ce fut inutilement; la jaune fit un nouvel effort, et le morceau fut avalé. Il ne parut dans ce combat singulier d'autres efforts que ceux qui tendoient à s'entre arracher la proie, et depuis les deux anemones toujours voisines ont vécu aussi paisiblement qu'auparavant.

Cependant les anemones sont quelquefois très avides. Croiroit-on que cet animal, qui peut vivre en assez bon état pendant une année, et peut-être plus, sans prendre d'autre nourriture que celle qu'il trouve disséminée dans
l'eau

may be diffeminated in the sea water, and does not seem very active to lay hold of it's prey, but rather waits patiently till chance throws it within reach of its limbs; that this animal should still be so greedy as to gulp down two whole muscles, I gave one of them by piece-meal, and burst the next day with indigestion, when it has a power of throwing up so easily what it has swallowed down? This was the case with an Anemone of the third species, and of a middle size, which had been fished lately.

On the 8th of October, the same Sea-Anemonies (of which I had clipped out the upper part with the mouth and limbs, instead of which new ones were re-produced, perfect enough to enable them to eat) were divided a second time, and again renewed, so as not to be distinguished from those which had undergone no operation. More than

l'eau de la mer, et qui ne paroît pas se donner de grands mouvemens pour attraper la proie qui l'environne, qui au contraire attend patiemment que le hazard la fasse tomber entre ses membres, soit assez glouton pour avaler en deux heures la valeur de deux grosses moules, qu'on lui présente par morceaux et crever d'indigestion le lendemain; tandis qu'il peut rendre aisément ce qu'il avale? C'est ce qui est arrivé à une anemone de la troisième espèce et de moyenne grosseur, qui étoit récemment pêchée.

Le huit Octobre les mêmes anemones de mer, dont j'avois retranché la moitié du corps où se trouvent les membres et la bouche, et auxquelles toutes ces parties s'étoient reproduites, jusqu'à leur permettre de manger, ont été coupées de nouveau et ont repoussé pour la seconde fois si bien, qu'il n'est plus possible de les distinguer de celles qui n'ont souffert aucune al-

than that, having taken particular care to feed one of these halves clipped for the second time, I saw it grow stronger and stronger every day, and perform with equal facility the same functions as any other compleat original Anemone. The only difference or exception is, that its basis is not yet perfect enough, as to enable the animal to adhere or fix itself to the glass; I make no doubt however, but what this new Anemone will, in a short time, acquire the only powers yet wanting, to render it a perfect one; see fig. 16 and 17. Might not one ask, upon all these facts, what is become of the original animal? is it that which continued adhering by its basis to the glass? or is it the upper half? Are there animals among which an *individual* is not a *simple* being?

There oozes habitually out of these insects an excrementitious humour, which thickens round their body. At periodical times it issues out in a greater quantity, and assumes then a
vermi-

teration. Il y a plus, ayant pris soin de nourrir une des parties retranchées, par la seconde coupe, je l'ai vue se fortifier de jour en jour; et faire avec la même aisance toutes les fonctions de l'anemone parfaite, excepté que la base n'est point encore en état de s'attacher, mais elle s'élargit et je ne doute point par les progrès que je lui vois faire que la nouvelle anemone n'acquiere dans peu le seul point qui lui manque, pour arriver à l'état parfait, voyez les figures 16 et 17. Ne pourroit-on pas demander maintenant qu'est devenu le premier animal? est-ce celui qui a été retranché, est-ce l'autre? y auroit-il donc des animaux où un individu ne seroit point un être simple?

Il sort habituellement du corps de ces animaux une humeur excrémentitielle qui s'épaissit; mais elle en sort aussi périodiquement en plus grande abondance, et y prend alors une
forme

cular appearance. During that period, the Anemone seems to be sickly ; but afterwards recovers its strength.

It is upon this species that I had particular opportunities of observing, that when, by cutting, there remains but the basis, and a short stump of the body ; the animal is equally, if not more, affected by the light, than before the operation. The same effect is produced upon the part where the mouth and limbs are.

I may perhaps be taxed with cruelty, with regard to these insects. But, I believe, I shall soon be absolved of the charge, when it is considered what favourable effects my experiments have had upon the Anemonies which have been the subject of them ; as I have not only extended their existence, but likewise renewed their youth ; and this sure is no small advantage.

What

forme vermiculée ; pendant ce temps l'anemone paroît malade, mais après elle reprend une nouvelle vigueur.

C'est particulièrement sur cette espèce que j'ai eu lieu d'apercevoir que lors même qu'il ne reste à ces animaux que la base et un tronc assez court, ils sont autant et plus affectés par la lumière, qu'avant d'être mutilés. Il en est de même de la partie, où sont les membres et la bouche.

On pourroit être tenté de m'accuser de cruauté envers ces animaux, mais si l'on fait attention aux effets qu'ont produit mes expériences, ou soupçonnera aisément que plusieurs ne peuvent que se féliciter d'en avoir été l'objet ; car non seulement j'ai étendu leur être, mais probablement je les ai rajeunis, ce qui n'est pas un petit avantage.

What I have mentioned of the effects of fresh water upon the first species of Sea-Anemonies, of the colour of the *re-produced* limbs, of their way of swallowing whole live muscles, &c. must be likewise understood of, and applied to this third species; and, in many respects, to the second.

I am sensible that many things are still wanting in this paper; but what I have wrote down, is very near all that I can infer from my first observations and experiments, without exposing myself to the necessity of falling back in regard to essential things.

POST-

Ce que j'ai dit des effets de l'eau douce sur la premiere espece, de la couleur des membres reproduits, de la maniere d'avaler les moules, &c. doit s'appliquer à celle-ci, et en grande partie à la seconde.

Je laisse sans doute beaucoup à desirer ici, mais ce qui y est expose est presque tout ce que je puis conclure de mes premieres observations et de mes premieres expériences, sans risquer de me mettre dans la nécessité de revenir sur mes pas pour des choses essentielles.

POST

P O S T S C R I P T.

Havre de Grace, April 28, 1773.

JUST as I concluded this essay, I found out a *fourth* species of Sea-Anemonies, of the size of the second, and of an elegant form, having the appearance of a cluster of white or flesh-coloured feathers. These Anemonies are found in oyster-beds, &c. I have observed that there grows or comes out of their body and mouth a sort of threads about the size of a horse-hair, which being examined with a solar microscope of five inches diameter, appear as if made up of a prodigious number of vessels, wherein a liquor is seen to circulate. The largest

P O S T S C R I P T U M.

Le Havre, 8 Avril, 1773.

LORSQUE je terminois ce mémoire, j'ai découvert une quatrieme espece d'anemones de mer, grosse comme la seconde, et d'une forme élégante, représentant comme un assemblage de panaches blancs ou couleur de chair. On la trouve dans les huitrieres, &c. J'ai remarqué qu'il sort de son corps et de sa bouche certains cordons de même couleur déliés comme un crin, lesquels vus au microscope solaire sous un diamètre de 5 pouces, paroissent formés d'une quantité innombrable de vaisseaux où circule une liqueur. Les plus gros

largest of these unite together, much in the same manner as the optic nerves do in man. Such an organization is doubtless intended for most important purposes. Some young ones of this species, which still adhered to one another by a string of communication, shut themselves up in the same instant, when this string was touched in the middle. As I could not directly contrive a total section of this large species, I tried it upon the young ones; and these shooted out again after the operation; (and so have the old ones done since). I have met with a sort of monster among these Anemonies, viz. one which seemed to inclose or contain three others, two of which were united at their basis, and the third lay, as it were, concealed in the folds.

Nature has resources little known to us; it seems sometimes to vary its operations, with an intent, as it were, the more to stimulate our curiosity

s'unissent en se renoutrant à peu près comme les nerfs obliques dans l'homme; une telle organisation est sans doute destinée à des usages précieux. Des petits de cette espèce, qui étoient encore adhérens l'un à l'autre par un cordon de communication, se fermoient en même tems lorsque je touchois ce cordon par le milieu. Ne pouvant d'abord soumettre cette grande espèce à une section totale, j'en ai essayée sur de jeunes individus, ils ont repoussé; (les grands repoussent de même). J'ai eu dans cette espèce un monstre, c'est-à-dire une anémone qui paroïssoit en contenir trois, dont deux opposées par leur base et une troisième cachée dans les plis.

La Nature a des ressources qui nous sont peu connues, et semble quelquefois varier ses procédés à dessein de piquer de plus en plus notre curiosité, et peut-être de dévoiler les secrets, ceux

riosity, and perhaps to disclose her secrets to those, who are endowed with a degree of sagacity or patience, sufficient to follow and investigate the effects offered to our observations. Amidst the great number of Anemonies of the third species which I have clipped across the body, there have happened to be two, whose lower part has in the usual way shot forth new limbs: but the *upper half*, where the limbs and mouth were, instead of healing up into a new *basis*, has produced both another mouth and limbs. Hence an animal was formed, which caught its prey, and fed at both ends in the same time.

The Sea-Anemonies of the three first species mentioned before, and perhaps those likewise of the fourth, feed upon those floating transparent animals of a white glassy, or of a blue or purplish hue, called *wandering nettles*, or *sea-jellies*. An Anemone of a middle size, of the first and third species,

ceux qui ont assez de sagacité, ou même de patience pour suivre les effets qu'elle nous présente. Dans le nombre considerable d'anémones de la troisième espèce que j'ai coupées par la moitié du corps, il s'en est trouvé deux, dont la moitié inférieure a repoussé de nouveaux membres comme à l'ordinaire: mais la moitié supérieure où étoient les membres et la bouche, au lieu de se consolider et de former une base à la section, y a reproduit des membres et une bouche, de sorte qu'il en a résulté des animaux, qui saisissent la proie, et mangent par les deux bouts en même temps.

Les anémones de mer des trois premières espèces, et peut-être ceux de la quatrième, se nourrissent de ces animaux flottans et transparens, le plus souvent de couleur de verre blanc, ou bleus, ou violets, connus sous les noms d'*Orties errantes*, de *gelées de mer*, &c. Une anémone de la première et de la troisième espèce

comme

species, such as that represented by Figures 1, and 13, swallows one of these animals of the size of half an orange. All these four species are good to eat.

Particular EXPLANATION of some of the FIGURES.

Plate XVI. Fig. 14. shews the Anemone of the third species, when shrunk up. One sees round it a ring of sand and broken pieces of shells sticking together by means of the excrementitious humour habitually oozing out of the body of the animal, or out of the little granulated knobs, with which it is covered towards the upper part. This ring is also to be seen in the same Anemone, when lengthened out, as expressed in Fig. 13.

Fig.

comme celles des figures 1 et 13. avalent un de ces animaux gros comme la moitié d'une moyenne orange. Toutes ces 4 especes d'anemones de mer sont très bonnes à manger.

EXPLICATION de quelques unes des FIGURES.

Planche XVI. On voit à la figure 14 une anemone de la troisieme espece, dans son état de contraction. L'espece de cordon, qui paroît tout autour, est formé de sable et de fragmens de coquillages joints ensemble par cette humeur excrémentitielle, qui sort habituellement du corps de l'animal, ou des petits mammelons dont la partie supérieure est revêtue. Cet anneau s'apperçoit aussi dans la même anemone, lorsqu'elle s'allonge, fig. 13.

Dans

Fig. 10. shews a Sea-Anemone, of the second species, concealed under the sand, and covered over in different places with broken shells and gravel, with which the animal forms a coat of mail to secure itself under, but out of which it can slip in an instant. The figure shews it when it spurts out water at its mouth, and at the end of its limbs.

Plate XVII. Fig. 11. shews the same Anemone open. The mouth is in the center of the upper part: it is not always shaped in the same manner in other Anemonies as it is seen here, or at least does not always appear to be so. Figure A shews a mouth as engraved from another Anemone, but which alters or shifts its form every moment. This Anemone has got five rows of limbs. There are ten of them in the innermost row; the like number in the second; twenty in the third; thirty in the fourth; and fourscore in the fifth.

When

Dans la figure 10 de la même planche se voit une anémone de mer, de la seconde espèce, cachée dans le sable, et couverte en divers endroits de coquillages brisés, et de gravier, dont cet animal se fait une espèce de côte d'armes, sous laquelle elle se cache, mais dont elle peut sortir en un instant. La figure la représente dardant de l'eau de sa bouche, et de l'extrémité d'un de ses membres.

La figure 11 de la XVII. planche montre la même anémone ouverte. La bouche est au centre de la partie supérieure: cette bouche n'est pas toujours formée de la même manière dans d'autres anémones, ou du moins ne le paroît pas toujours. Vous voyez en A une bouche dessinée d'après une autre anémone, et qui à chaque instant change de forme. Cette anémone a cinq rangs de membres: le rang intérieur et le suivant en ont chacun dix; il y en a vingt dans le troisième, trente au quatrième, et 80 au cinquième.

Quand

When the animal is out of the water, and is squeezed, it spurts out water at the mouth and at several of its limbs at the same time; so that it imitates pretty well the play of water-works. When the limbs are drawn in closer together, they give it the look of a flower, especially of an Anemone.

Fig. 12. shews an Anemone of the same species, turned inside out, as when a purse or stocking is so. A thin transparent membrane, with white stripes, lines the whole inside of the animal; and through it are seen the bowels, part of which hang or come out at the middle. One may observe, besides, in this figure, two hollows sinking in, which are formed by two pretty strong cartilages.

Quand l'animal est hors de l'eau, et qu'on le presse, il jette de l'eau par la bouche, et plusieurs de ses membres en même tems, imitant assez bien ainsi le jeu des jets d'eau. Lorsque les membres sont ferrés l'un sur l'autre, ils donnent à l'animal la figure d'une fleur, et surtout d'une anemone.

A la figure 12, de la même Planche, vous voyez une anemone de la même espece, mais tournée du dedans en dehors, comme un bas ou une bourse. Tout le dedans est revêtu d'une membrane fine et transparente, avec des rayes blanches, les intestins paroissent au travers, et une partie sort du milieu et pend au dehors. On voit aussi deux ouvertures, qui penetrent dans l'intérieur, et sont composées de deux cartilages assez forts.

Fig 11

A



Fig. 12.



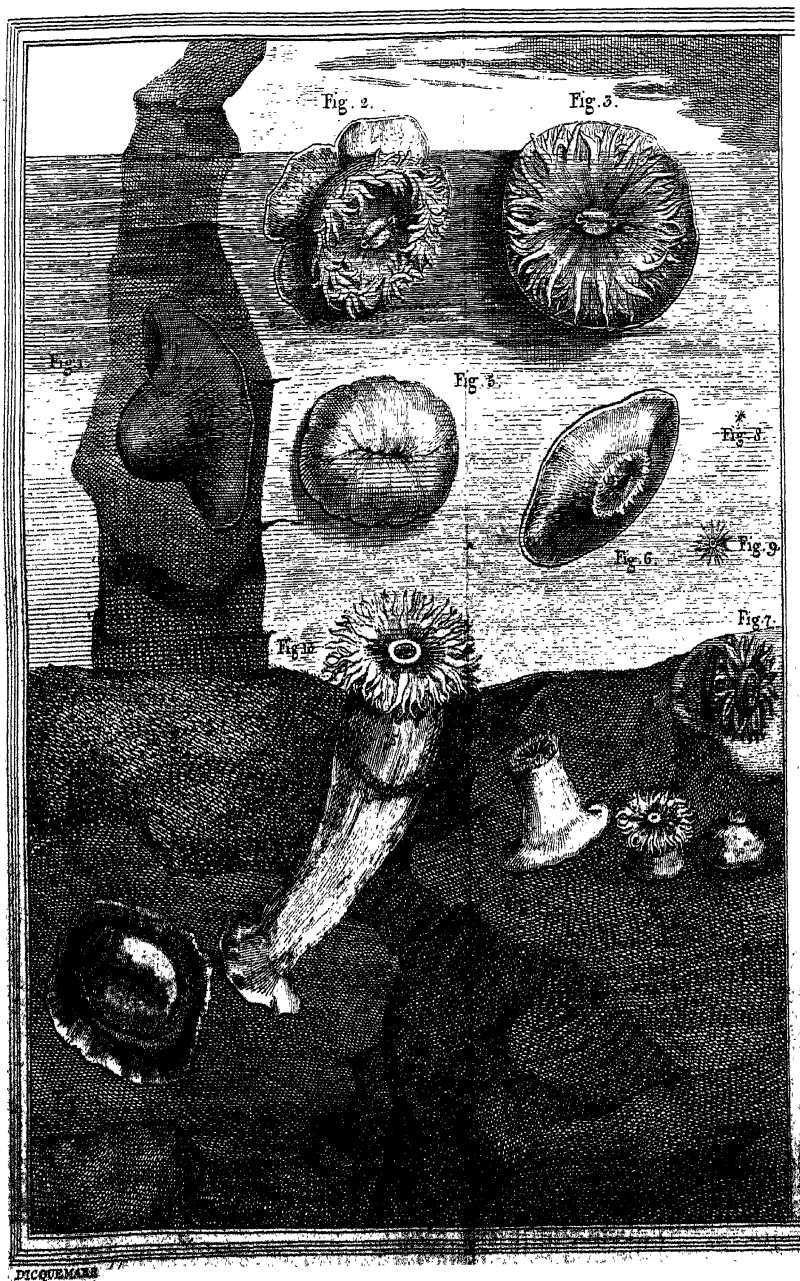
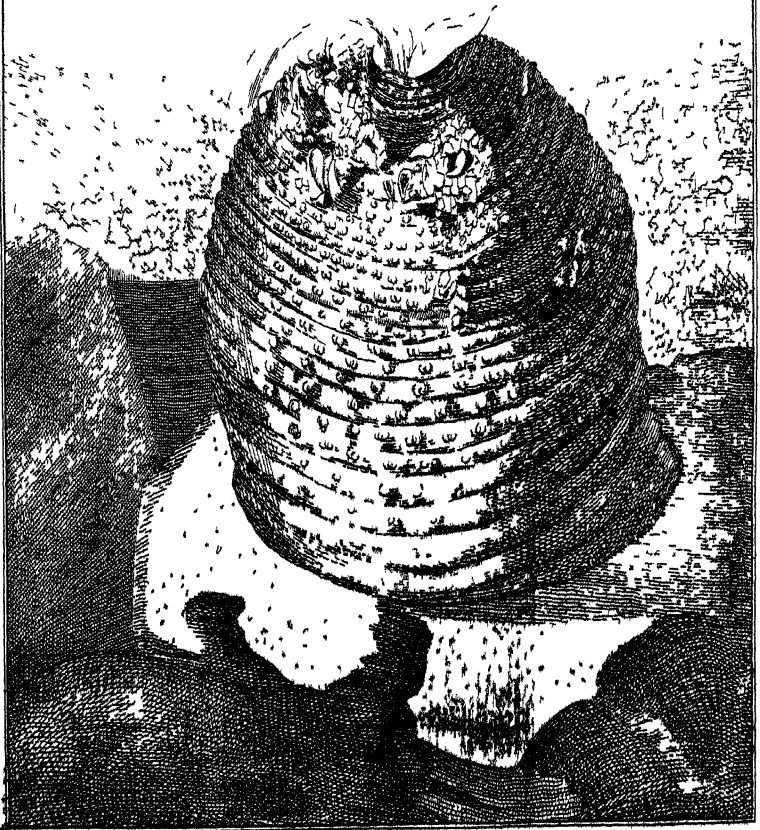


Fig 10



N.B. Dr. Solander, F.R.S. being consulted about these sea-worms, which are evidently of the class of the *Actinia*, referred the first species, Tab. xvi. Fig. 1—3. to the *Actinia Equina* Linn. Syst. Nat. 1088. 1. the second species, Tab. xvi. Fig. 10. and T. xvii. Fig. 11 and 12. to the *Actinia Senilis*. Ib. 1088. 2. and the third, Tab. xvi. Fig. 13 and 14. to the *Actinia felina*. Ib. 1088. 3.

XXXVIII. *Account of a new Hygrometer.*
By M. J. A. De Luc, Citizen of Geneva,
F. R. S. and Correspond. Member of the
Academies of Paris and of Montpellier.

Read June 10, 1773.

IN laying before the Royal Society an account of my attempts to find out a method for measuring the moisture of the air, I think myself obliged to relate the gradual steps of my mind, the obstacles I met with, the means by which I endeavoured to overcome them, the degree at which I flatter myself to have arrived, the hopes that may be entertained of farther advances, and the uses which may be derived from my first experiments.

Attempts to invent an HYGROMETER.

1. In order to proceed regularly in this investigation, I began by examining the essential requisites in a machine intended to measure humidity, which I found to be the three following:

1st, The settling of a fixed point, from which every measure of the same kind should be taken, such, for instance, as that of boiling water in a thermometer, when the barometer is at a certain height.

2d, Degrees equally determined, or comparable, in different hygrometers, such as are in the thermometer,

meter, the scales of Fahrenheit, Delisle, Reaumur, &c.

3d, Constancy in the variations produced by the same differences of humidity.

2. I perceived, moreover, that it were to be wished, that the hygrometer should give a true indication of the relation between the real quantities of the humidity, or at least between their differences: but this last point I rather considered as a desirable degree of perfection, than as an indispensable requisite; the essential point being, that observers might understand each other, when mentioning degrees of humidity; and this seemed to be sufficiently provided for by the abovementioned conditions.

3. Having thus planned to myself the work I had to go through, I first attended entirely to the first point, and laid aside all the others. This I again subdivided. I had soon perceived that I must begin by thinking much less of the hygrometer than of the different phenomena of humidity. For this purpose it was necessary to find out a fixed state, either of bodies in general, or of some body in particular; and this fixed state might either be extreme humidity, or dryness, or any intermediate point.

4. Knowing that the extremes in nature are commonly very difficult, and sometimes impossible to hit, I conceived at first greater hopes of intermediate degrees. But in vain did my imagination fatigue itself in a road, which I was forced to abandon.

5. I then came to the extremes, and that of absolute dryness was the first I was induced to try. But having found no other way to procure it but by fire, and fire not producing it in all bodies which appeared

to me susceptible of humidity, but by altering their nature, I reluctantly perceived that I should be obliged to look for my first point, where I had the least hopes of discovering it.

6. I remained a long time without discovering any thing in this new road ; and very often turned back, but was always obliged to return to extreme humidity, as to the only part of my object, of which I could possibly get any hold.

7. The words, which are necessary for communicating our ideas to others, are often obstacles to the raising of new ideas in ourselves. They are by far too few to express distinctly every shade of intellectual objects. Humidity was a word which I constantly repeated to myself, and it constantly led me to a class of phænomena, in which I could find nothing settled.

8. Water at length presented itself to my mind ; and in this fluid, which to all appearance ought first to have struck me, I beheld with surprize, what I had been labouring, through many a round, to discover, under the denomination of extreme humidity. I was not at that time considering humidity in any particular phænomenon ; I only observed that it was constantly produced by aqueous particles diffeminated through bodies ; and I found in water the maximum of the approach, and consequently of the action, of these particles.

9. In order now to avoid the ambiguities from whence, in my opinion, the difficulties in these matters arise, let me be allowed for the future to employ no words but such whose meaning is well determined. Humidity will accordingly be no more
that

than an effect, or modification of bodies from a substance more or less abundant, but constantly consisting of aqueous particles under different forms. This substance, considered in its utmost extent and under all the appearances which it assumes in nature, I shall express by the Latin word *humor*. Thus ice, water in its different degrees of heat, hail, snow, icicles, rain, dew, clouds, fog, mist, invisible vapours, are no more than modifications of this same substance, different species of a determined genus; since aquosity, which is common to all, is its generic character.

10. The more humor there is in any body, the more humid that body is; and consequently if it be plunged in water, and soaked so as not to be able to receive any more, it is got to extreme humidity, the water which fills up all its pores being humor in the highest degree of intensity.

11. Not, however, but that discrete humor, or vapour of every kind, may in some respects produce as great effects as concrete humor or water: but there is always some difference in some other respect, and chiefly in regard of time. Bodies encompassed with air are continually discharging, by evaporation, part of the humor they imbibe from it. If the circumstances are such, that the humectation exceeds the evaporation, the body at length wets through*, more or less quickly as the quantity of humor which it receives in a given time is greater or less, and likewise in proportion as this quantity exceeds that

* By *wetting* here, I understand arriving at the greatest degree of humidity.

which evaporates. It is suddenly wetted, when the humor is so condensed as to become water, because the evaporation which takes place at the surface of the water, does not weaken its action on the bodies dipped into it; it is only wetted little by little, or, what happens ofteneft, in part, when the humor is discrete or reduced into vapour; because, while it is deposited on particular spots, it evaporates from the interstitial parts, and that more or less according to the state of the air, and that of the moistened bodies.

12. This difference, however, in point of time, between the action of concrete and discrete humor, only takes place on the surface of bodies, or at a small depth; it diminishes, and may even become opposite, as the depth of bodies increases, because the discrete humor is then more easily introduced into their pores than water, which more than makes up for their different intensity.

13. This consideration solves a difficulty, which at first puzzled me. I had been told by bird-catchers, that the threads of those nets which they cast on the water-side, were less stretched from the action of water than of dew. Hence it might seem, that what I took for the extreme of humor had less effect than what is only a degree of it. But two particular causes accounted for this difference.

1st, The air contained within the fibres of the thread opposes the introduction of the water, which, presenting itself in a body, shuts up the passages by which the air should escape to give it room; but it yields to the drops of dew, which permit its escape while they penetrate through the threads.

2^d, A o h e r

2d, Another particular cause, less obvious though not less probable than the preceding, is the difference in the mutual attraction of parts, in the concrete and discrete humor, and consequently in their respective facility to separate, and get one by one through the narrow pores. When this entrance is attempted by the humor, under the form of water, the mutual attraction of its parts, being greater than in dew, occasions a greater resistance to their introduction, than when they are already divided by some other cause, viz. when the humor is reduced to small drops, or vapor.

14. This phenomenon, therefore, does not contradict my principle; it is only a particular fact; and it remains true, that bodies surrounded with water are exposed to the extreme of humor. To remove this cause of exception from my hygrometer, it was sufficient to provide outlets for the air, and not to increase too much the thickness of the body, upon which the humor was to act.

15. Another difficulty, which presented itself, was that water might probably act with more or less energy in proportion to its heat. But this did not stop me long. As my present object was a fixed point for the hygrometer, and not the greatest power of water, considered as a cause of humidity, it was enough to employ it constantly at the same degree of heat; and, to fix this with greater precision, I determined to use water at the instant that it ceases to be ice. The basis therefore of my hygrometrical scale was to be the soaking power of melting ice.

16. This principle, being thus unfolded, appeared so simple, that I was at first surprised how it could
have

have been so long overlooked. But I afterwards accounted for this, from the difficulties which I met with in the discovery. The notion of an hygrometer being both complex and unsettled, all the obstacles presented themselves at once, and this multitude of ideas exceeded the power of attention. The very first steps were apt to mislead. On the one hand, I looked for an hygrometer with a head full of the matters already used for hygrosopes, which always are more or less altered by water; and, on the other, the name of humidity was applied to that cause, the effects of which I wished to measure; and both points of view turned aside the mind from the idea of water, as being proper to afford the required fixed point in an hygrometer.

17. The first difficulty had not escaped me; but, considered in itself, it did not appear unfurmountable. I was in hopes that a substance might be found capable of being affected by the soaking power of water, without being altered by it. As the nature of this substance was to determine not only the form of the hygrometer, but also the species of the degrees, which were to indicate the different quantities of humor, I concluded that my second object ought to be the discovery of this substance.

18. In this research, I again divided the objects, by considering separately the three kingdoms, viz. the mineral, the vegetable, and the animal. The two first offered no substance fit for my purpose, viz. none that would obey the impressions of humor, without being altered either by it or by other causes. But in the animal kingdom bones drew my attention; and ivory, in particular, seemed to possess the
required

required qualities. I had observed that the key of an ivory cock was tighter or flacker, as there was more or less humor in the air. Ivory pallets, used for water-colours, shewed no alteration, at least none that was lasting. I knew also the elasticity of this substance, which seemed to secure its coming back to the same state, on its return to the same degree of moistness.

19. There still remained on this second head another object of inquiry, which was almost necessarily connected with the third, viz. the species of the degrees to be given to the hygrometer. The best form to be given to the ivory, in order to receive with ease the impressions of the humor, and to have its effects measured upon it, was to be determined. I first thought of ivory rods, the lengthening of which should be measured by a machine similar to the pyrometer. I likewise had some notion of a large nonius, formed of an ivory and a metallic rod. Either of these machines would admit of a fixed graduation, as both the dimensions of their parts and proportions to one another could be determined. But then I apprehended that ivory might perhaps, like wood, have its longitudinal fibres but little liable to be extended by the humor, and that the imperfections of these two kinds of micrometers would occasion a considerable irregularity in the hygrometrical degrees. I also feared that if ivory rods were made thick enough to prevent their bending, such a thickness might become an obstacle to their intire penetrability by the humor (14). I therefore concluded that the ivory should have such a form, that, though very thin, it might not warp; and that the measurable varia-

tions were to be the removal or approach of its fibres to each other.

20. Being thus guided by these necessary conditions, I thought of different thin ivory cups, the capacities of which should be measured by quicksilver; and at last imagined a hollow cylinder, in which the variation of its capacities, when more or less moist, might be measured by the quicksilver it should be filled with; and which putting into a glass tube joined to the ivory one, would of course rise more or less, as that vessel was more or less deprived of humor.

21. Nothing now remained but to find out a way of estimating the changes of capacity of the ivory tube, by means of the variations in the height of the mercury in the glass tube. I thought, at first, that by using very nice scales, in order to compare the weights of the mercury contained in the cylindrical vessel, with that of a column of the same liquid in the tube, I might obtain the proportions of these weights with a sufficient exactness, to be able to measure the variations of the mercurial column, by degrees representing aliquote parts of the whole mass.

22. This in itself was undoubtedly an exact method; but then it required in the execution such a nicety in the scales, that I durst not employ it in the construction of an instrument of so extensive an use. Such scales are always scarce from their high price. I remembered to have myself found that inconvenience in the construction of a Delisle's thermometer, and concluded I must hit upon some method

23. The idea of a thermometer, which struck my mind, was a lucky one. I was led to it by a kind

kind of connexion between the scale of that instrument and that of my hygrometer. I soon perceived, that, by applying to my hygrometer a thermometrical tube, already graduated by means of two fixed points of heat, it would only be necessary to know the proportion of the weights of mercury in this thermometer, and the hygrometer, to which its tube was to be applied, to have in this last instrument degrees as well determined as in the first. Scales of a common degree of exactness were sufficient to establish between the respective degrees of both instruments a proportion equal to that of their mercurial weights (42 and 43).

24. Besides the ease in the execution, this contrivance afforded me a very simple method to correct the effects of heat upon the mercury contained in the hygrometer. It is indeed obvious, that, abstractedly of the effects of the humor, the new instrument must in itself be a regular thermometer; and that consequently the variations of an adjoint thermometer were immediately to point out this correction.

25. Every principle being thus settled, nothing remained but to contrive its construction. I began by making some experiments, on the nature and quantity of the action of water upon ivory. I made for that purpose a small cylindrical ivory vessel, of an inch in diameter, and eight lines in length, and reduced its thickness to less than $\frac{1}{4}$ of a line. I likewise prepared a wooden cylinder, equal in its diameter to the internal one of the vessel: I then put this vessel into water, in such a manner that it only wetted it outwardly to the rim. In a very short time the wooden cylinder, which at first filled the

cup exactly, no longer filled it. After a few hours, I perceived that the internal surface grew wet, and by means of a magnifying glass, found it covered with a very fine dew. This dew did not encrease by the vessel remaining any longer in the water; the evaporation being doubtless equal to the transudation; and the capacity of the vessel, which encreased till the appearance of the dew, seemed afterwards at a stand.

26. This transudation puzzled me a little; it shewed me that the water would get into my hygrometer, which at first appeared an inconvenience. I soon, however, found an advantage in it. The water, after having soaked through the ivory, would immediately push back the mercury, which having by degrees sunk in the tube, during the penetration of the water through the pores of the ivory, must thus rise again. Hence I might expect a maximum for the fall of the mercury, very easy to be determined. As for the water introduced into the ivory vessel, I was in hopes that it would go back as soon as the outside of the cup should be dry.

27. Having thus ascertained that ivory was very easily affected by the impressions of the humor, it still was necessary to know, whether the variations of the one would always equally answer to those of the other. Having accordingly taken my small cup out of the water, and exposed it to the air, I soon found that its capacity diminished, but that even after several days it did not return to its former state. This again puzzled me; but I suspected that the external pressure of the tool upon the ivory might somewhat have compressed it, and that the water having restored the ivory fibres to their original pitch, the
absolute

absolute capacity of the cup remained larger than it was before.

28. To satisfy myself about this, I got another wooden cylinder, which filled the capacity of the vessel in its present state. This I again put into the water, and left it there a sufficient time; I then exposed it to the air to be dried; and after that found that the wooden cylinder filled it as before. Hence I concluded, that in the construction of my hygrometer the ivory cup should be dipped for some time in water, and afterwards dried, before it was used.

29. Thus having cleared up my conjectures, as much as they could be, by these preliminary experiments, and got some insight into the proportions of the different parts of the machine, I proceeded to its construction, and finished it in the following manner:

Description of an HYGROMETER.

30. Tab. XVIII. figure 1. shews the section of the interior part of the instrument, of its true length, in the direction of its axis.

The first part to be described, being in some measure the soul of the hygrometer, is an ivory tube, *aab*, open at the end *aa*, and closed at *b*. It is made of a piece of ivory, taken at the distance of some inches from the top of a pretty large elephant's tooth, and likewise at the same distance from its surface, and from the canal which reaches to that point (68). This piece is to be bored exactly in the direction of its fibres; this hole is to be very straight, and its dimensions are $2\frac{1}{2}$ lines in diameter, and two inches 8 lines in depth from *aa* to *c*.

31. Prepare

31. Prepare after this a brass cylinder, about $3\frac{1}{4}$ inches long, and to one of its extremities fix the pully proper to receive the string of the bow when the piece is turning. This cannot be done too carefully, both to make it perfectly round, and to fit it exactly to the hole of the ivory tube; its extremity must even be rounded, that it may be applied closely to the bottom of the hole. Having then roughly prepared the outside of the ivory tube, and introduced into it the brass cylinder, put both pieces thus united upon the turning wheel, and find out on the outside bottom of the ivory tube, the point which answers to the axis of the brass piece, in order that this may turn exactly upon its axis. It is with this view that the brass cylinder is made longer than the ivory tube.

32. All these precautions are designed to make the sides of this tube of an equal thickness, viz. $\frac{3}{16}$ of a line; except at the two extremities. At the bottom *b* the tube ends in a point, and at the top *a a*, it must for about two lines be left a little thicker, in order to enable it to bear the pressure of another piece, which is to be put into it. Thus the thin or hygrometrical part of the tube will be reduced to $2\frac{1}{2}$ French inches, including the concavity of the bottom.

33. Before this piece is used, put it into water so as that the external part alone be wetted by it, and leave it there till the inside be every where covered with the dew I mentioned before (25). This will take place in a few hours; I have given the reasons for this operation (28).

34. The glass tube intended for this hygrometer must be about 14 inches long. Its lower end is seen
in

in *dd ee* (fig. 1.). Its internal diameter is about $\frac{3}{4}$ of a line. The reason why it should not be sensibly less will be given hereafter (52); and if it was sensibly larger, the variations of height in the mercurial column would not be considerable enough. On the dimensions that I propose, when the hygrometer is put into melting ice, in a fine summer day, the mercury falls about six inches in the tube. The outside diameter of this tube should be about two lines, in order that the part *gg* of a brass piece through which it passes, and which is to enter into the ivory pipe, be as thin as possible.

35. The glass tube, as I said before, should have belonged to a thermometer. Its extremity widens of course towards the ball; which will be of use, when the mercury is poured into the hygrometer, in order that it may drive the air before it, by rising from the ivory pipe into the glass tube. To preserve this widening, break the ball of the thermometer by striking against the bottom; and with pinchers take off the rest little by little, and make the extremity cylindrical by grinding it upon the wheel. The same must be done at the top, which I suppose to have been made to end in an olive or small reservoir for the filling of the thermometer. This widening is likewise to be saved for the reasons hereafter to be mentioned (52).

36. The piece *ff gg* is intended to join the ivory with the glass tube. It is of brass, shaped as in the figure. A cylindrical hole is bored through it, which holds the glass tube as tight as possible, without danger of breaking it; and its lower part is to enter with some degree of difficulty into the ivory pipe.

37. To

37. To hinder the part of that tube, which encloses the brass piece, from being affected by the variations of the humor, which might sometimes prevent a sufficient pressure, I cover this part of the tube with a brass vessel, represented in *bb ii*. It must enter with force, and will henceforth be considered as part of the ivory pipe.

38. To unite those pieces together, I make use of gum lac, or of mastich, which melts by the heating of the glass and the brass. I first cement the brass piece with the glass tube by introducing the tube, and leaving it at first at an inch distance from the place where it is to be fixed; I then hold this end of the tube over live coals, by bringing it nearer and nearer, and turning it, that both that and the brass piece be every where equally heated; and when they are hot enough to melt the gum lac, I rub the glass tube with it, and push the brass piece to its place by means of a hollow bit of wood, drawn beforehand over the tube for this purpose. As the brass piece advances, the lac accumulates towards the end of the tube; I take away the superfluous part, but leave a slight coat of it over the end of the brass piece, in order to preserve it from the contact of the mercury that might corrode it. When this piece is properly placed, and still warm, I cover with lac its cylindrical outside, and introduce it into the ivory tube, which has been somewhat warmed by holding it near the fire, in order that the lac may stick more closely to it. As soon as these pieces are cold, they are found very strongly cemented together, and neither mercury nor water can make their way between them.

39. The introduction of the mercury is the next operation. I first roll a slip of paper three inches wide over the glass tube, and tie it fast to the extremity which is nearest to the ivory pipe. I then introduce into the tube a horse hair long enough to enter the cylinder by one end, and to have the other rise three or four inches beyond the orifice of the tube. I then raise the paper which has been shaped round the tube, and use it as a funnel to pour the mercury into the instrument, which I hold upright. The purest quicksilver ought to be employed for that purpose, and it will therefore be proper that it should be revived from cinnabar. I poured it then into the paper funnel, from whence it easily runs into the tube, with the assistance of some gentle shakes. The air which it drives before it comes out along the horse hair. Fresh mercury must from time to time be supplied, to prevent the entire emptying of the paper tube, and the running in of the mercurial pellicle, which the contact of air always produces upon the surface.

40. Some air bubbles generally remain in the tube; they may be seen through the ivory pipe, which is thin enough to have some transparency. These being collected together by shaking, must be brought to the top of the tube, and expelled, by means of the horse hair. To facilitate this operation, some part of the mercury must be taken out of the tube, in order that the air may be less obstructed in getting out, and the horse hair have a freer motion to assist it.

41. Air, however, cannot be entirely driven out in this manner. It is the weight of the mercury,

with which the tube is for that reason to be filled, that in time completes its expulsion, by making it pass through the pores of the ivory. To hasten this, I place my hygrometers in a box made on purpose; and this I fix pretty nearly in a vertical direction, to the saddle of a horse, which is set a trotting for a few hours. The shakes sometimes divide the column of mercury in the glass tube, but it is easily reunited with the horse hair. When, upon shaking the hygrometer vertically, no small tremulous motion is any longer perceived in the upper part of the column, one may be sure that all the air is gone out.

42. I now come to the operations requisite to make the scale of the hygrometer, and first of all to that which determines the base (15). This may be done as soon as the air is gone out. I then suspend the instrument in a vessel filled with ice mixed with the water it produces in melting. I take care to supply the melting by recruits of fresh ice, during the course of this process, which lasts ten or twelve hours. In the first hour, the mercury sinks above one third of the space it has to go through; it advances less in the second; and its motion lessens thus gradually, till it appears stationary, which frequently happens after seven or eight hours, and it remains two or three hours in that situation. The ivory being then become more transparent on account of humidity, a very thin dew is perceived by a certain play of the light on the surface of the quicksilver. Lastly, the mercury begins to reascend; the operation is terminated; and small drops of water, as I expected, are at that instant seen upon its surface (26).

43. I follow the last steps of the mercury in its fall, by means of a fine silken thread fixed very tight around the tube. This is left at the lowest point it has been brought to. If this point be too low, relatively to the frame of the hygrometer, fresh mercury is poured in, and the thread proportionally drawn up higher; if too high, I take off some of the mercury and lower the thread; and in both cases make use of the horse hair. This must be done when the mercury ceases to fall, in order that the place where the thread is to remain may be immediately determined by this operation.

44. This point thus fixed is named 0 in my hygrometer; it is that in which dryness is nothing (if I may be allowed to express myself so), since it is that of extreme humidity, in a given heat; viz. that of melting ice. From this point are reckoned all the degrees I am now going to speak of; which thus become degrees of exsiccation.

45. The last essential operation is that by which the size of the hygrometrical degrees are determined; and this I shall describe by an example. It must be remembered that the hygrometer's tube was originally a thermometer (23). I take it in this first state, in the instance I am going to give. The distance between the thermometrical points of melting ice and boiling water, at twenty-seven French inches of the barometer, was found to be 1937 parts of a certain scale. I broke the bulb of this preparatory thermometer, in a basin, in order to receive carefully all the mercury that it contained. This being weighed in nice scales, amounted to 2 *li. 11 dr.*

12 gr. or 1428 grains. All the pieces of my hygrometer being put together, it weighed 373 grains, and when filled with the proper quantity of mercury 833. It consequently contained 460 grains of mercury.

46. By the rule above given (23), the extent of the hygrometer's degrees, ought to be to that of the degrees in the preparatory thermometer, in proportion of the respective weights of mercury in the hygrometer and thermometer; and consequently as the weight of the mercury in the thermometer is to the weight of the mercury in the hygrometer, so is any given interval in the thermometrical scale, to the corresponding interval in the scale of the hygrometer. Consequently in our example as $1428:460::1937:624$ (nearly); and the corresponding intervals on the scales of the thermometer and the hygrometer, ought to follow the proportion of 1937 to 624.

47. I call the distance between the two fixed points of heat in the thermometer the *fundamental interval*; and I shall call the *fundamental line* in the hygrometer that of which the length corresponds to this interval. Thus the *fundamental interval* in the preparatory thermometer, being 1937 parts of a certain scale, the *fundamental line* of my hygrometer consisted of 624 parts of the same scale. This example may so easily be applied, that it will be unnecessary to dwell any longer upon this subject.

48. Having thus got a *fundamental line* in the hygrometer, I had it in my power to divide it into as many parts as I thought proper: my choice was naturally

naturally to be determined by the simplicity of a proportion between the degrees of the thermometer, and those of the hygrometer, because this last was to be corrected by the first, on account of the effects of heat (24). My first thought was to divide this *line* into 80 parts, agreeably to the divisions of the fundamental interval in what I call the *common thermometer* in my book upon the air, which I shall always be understood to mean in this paper. But as the minuteness of these degrees was found to be inconvenient and superfluous, I determined to make them double, by putting only 40 in the length of my hygrometer's fundamental line. It is easily understood that these degrees, thus settled, begin to be reckoned from the place of the thread, which indicates upon the tube of the hygrometer extreme humidity, by the heat 0 of the *common thermometer*, or of melting ice.

49. The instrument with its frame is seen fig. 2. the dimensions of which are every way one half of those of the original. It is mounted on deal, that being the wood, which suffers the least change in the length of its fibres. The lower part of the frame is slit through the whole length of the ivory pipe, in order that the air may circulate freely round this pipe, and the bulb of a thermometer which I shall mention presently. The hygrometer is fastened in three parts; viz. at bottom on a small bracket, at top by a tube passing through a piece either of hard wood or of metal fastened by screws; but chiefly by means of a brass wire on the neck of the brass piece, which unites the glass with the ivory pipe. This piece is laid in a small plate of a hard wood,

wood, which in that place fills a groove originally made throughout the whole length of the lead-board.

50. To keep the dust from getting through the opening of the tube, I shut it up in a small ivory case. It cannot be sealed up, because if air was left in, it would obstruct the rising of the mercury; and if it was exhausted, the mercury would be pushed to the top by the pressure of the atmosphere upon the ivory pipe; as I have experienced it.

51. Hence however arises a small inconveniency; which is, that as the upper part of the column of mercury communicates with the air, if it remains long in the same part of the tube, or moves but little in it, some dirt may be left on the sides. This I easily remedy, by means of a brass wire, the extremity of which is dentated in the form of a file, in order to hold some bits of cotton, which I put round it. The wire is easily introduced into the tube, by means of the widening mentioned before (35). I put it in, when the mercury is below the part it has soiled, and easily clean it by this means. It is on this account that the tubes to be employed are to be of about $\frac{3}{4}$ of a line internal diameter.

52. The scale of the hygrometer is marked upon a deal slip, which slides along the groove I mentioned before (49). This, as well as all the other parts of the frame, must be lined with paper, to mark the necessary scales; and this paper is afterwards varnished over. Thin plates of silvered brass can be employed for the same use.

53. The mobility of the scale of the hygrometer serves to correct, in the observation itself, the effect

of the heat on the mercury. At the top of this scale is seen an index, over-against another small scale, marked upon the unmoveable part of the frame. The degrees of this small scale are eightieth parts of the fundamental line, and consequently immediately answer to the degrees of the thermometer on the same frame (48). When the index points to 0 of the small scale, the thread which indicates upon the tube of the hygrometer the point to which the mercury sunk in the melting ice, answers likewise to 0 in the scale of the hygrometer. This is the case expressed in the figure wherein the thermometer is likewise represented at 0 of its scale. By first observing the heat therefore, and conducting the index to the point of the small scale, which answers to the actual degree of the thermometer, the hygrometer will only indicate upon its scale the degrees of the humor. For this scale going through the same variations that the heat occasions in the height of the column of mercury, the indications of the hygrometer become just the same as they would be, if the heat always continued that of the point at which extreme humidity was fixed, viz. 0 of the common thermometer.

The scale of the hygrometer is carried to the proper point, by means of a knob fixed on a small piece of hard wood or metal, screwed to the bottom of the board, and which affords a free passage to the tube of the hygrometer.

*Account of the first OBSERVATIONS made on the
going of this HYGROMETER.*

Read June 10, 1773. 54. My first hygrometer was ready for observation at the beginning of last February (1772), in a rainy season. A few hours after it was taken out of melting ice it was already at 54 degrees of its scale. The next morning it was only at 50, but towards noon it rose again to 54. I carried it down to my cellar, which being a considerable depth under ground is commonly very damp. As I went down the stairs, I perceived that my hygrometer continued falling, so that when I hung it up in the cellar it was as low as 35.

55. In the evening of the same day it was at $28\frac{1}{2}$, and the next night at $21\frac{1}{2}$. It continued falling imperceptibly during the rest of this month, throughout the whole of the next, and till the 19th of April. On that day it was at $3\frac{1}{2}$, and consequently very near extreme humidity: but in this interval it had very often rained, and snowed, and even when the sky was clear over head, the streets had always remained wet, so that it was evident from all the common appearances, that the humidity had gone on considerably increasing in the cellar.

56. I was impatient to see the hygrometer rise again in the cellar itself, which I could not however expect but with a north wind. At length, on the 20th of April, though the rain still continued, the hygrometer rose half a degree. In the night of the 20th to the 21st the wind came about to the north, and when I looked at my hygrometer in the morning, I found it at $6\frac{1}{2}$. It continued rising imperceptibly

ceptibly the whole of that day, and the next morning stood at $9\frac{1}{2}$.

57. Another circumstance I was anxious to know, was, whether the hygrometer, after having been kept in the cellar so long, would rise, upon being carried up stairs again, to the point from which it had fallen. The importance of this new observation prevented me from pursuing that I had begun in the cellar. I therefore took my hygrometer out of it, and while I was going up the stairs it rose three degrees. This was at six o'clock in the morning. At seven it was already at 17, and at eight at $23\frac{1}{2}$. From eight to eleven it rose to 43, and at one o'clock stood at 63. After this it fell again, and at half an hour after five was no higher than 50. The sky had been clouded during the last interval.

As the preceding observations relate only to the hygrometer, and not to humidity, I shall confine myself to them. They are sufficient to give an idea of the going of the instrument in the season they were made. I shall hereafter give an account of some observations taken at other seasons.

FIRST EXPERIMENTS *made to discover the Accuracy of this INSTRUMENT.*

58. The most important thing after the preceding observations, was, to try whether the instrument was in reality comparable. To ascertain this, I immediately constructed four more upon the same principles, which were finished on the 23d of August.

59. I could not use my first hygrometer to make comparative observations with the new ones; its tube

being either too narrow or too short. The proportion I had settled between the capacity of this tube, and that of the ivory pipe, was deduced from the preliminary experiments I had made in the month of December (29); and had of course been found just, as long as the spring lasted. But even before the new hygrometers were completed, the quicksilver had risen in the first so as to run out of the top of the tube. This, joined to some other previous observations, which had convinced me that the diminution of the humor is much more considerable on mountains than in plains (76), induced me to fix the dimensions of the tube of the hygrometer in the manner laid down in the description of the instrument. I had been in time to follow these dimensions in the construction of my new hygrometers, so that when they were brought from extreme humidity to the state of the air in my apartment in the month of August, the quicksilver did not rise too high in them; that is, it remained sufficiently below the top of the tube, to indicate lesser degrees of humidity afterwards.

60. The four new hygrometers have been constructed with as little reference to each other, as it they had been made in different countries. By comparing them therefore, I have been enabled to judge of what might be expected from the agreement of instruments of this kind. This is what I have found.

When I have observed them in places where it appeared likely that the humor would be equally distributed among them, the utmost of their difference has been usually from 19 to 21. Their greatest height, for instance, in my room with the windows shut,

shut, has hitherto been $94, 99\frac{1}{2}, 100\frac{1}{2}, 105\frac{1}{2}$, in the same moment; which is pretty nearly in the proportion of 19 to 21, between the hygrometer which remains at the lowest, and that which is at the highest.

61. Besides this difference between the relative altitudes of these four instruments, I have observed another kind of irregularity in them, which is, that they do not always preserve the same proportion to each other. These variations are undoubtedly in part owing to the cause itself of their motions; that is, to the unequal distribution of humor even in places very near each other; but I have reason to ascribe part of them to some defect in the instruments themselves. I shall hereafter return to these causes, and give them a closer examination.

*CONSIDERATIONS on the Degree of ACCURACY
that has been observed.*

62. Notwithstanding the defects I have mentioned were rather evident, I was not dissatisfied with this first trial. I never imagined that I had foreseen every thing, and consequently could not expect to arrive at a sufficient degree of exactness without the help of experience; the irregularities therefore which appeared in the execution, did not make me despair of being able to perfect that instrument.

63. My hopes in regard to this were at first only grounded upon general reflections. I recollected what the barometer and thermometer had been when they first came out of the hands of their inventors; and observed that in some respects they were more

irregular than my hygrometer is at present. Though the first of these instruments was very simple in itself, yet barometers hung up in the same places used to vary three or four lines from each other. Some of the members of the French Academy have been themselves engaged in considering a barometer that always kept 18 lines below the rest, and they have formed various hypotheses to account for this difference. The variations therefore of the barometer, though observed only in the same place, were much greater than those between my hygrometers.

64. Nor was the thermometer itself, which is now brought to such a degree of accuracy, much superior at first to our hygrosopes, for the purpose of comparative observations. The first philosophers who treated of it knew nothing of any fixed point or determined degree in it; they knew nothing even of the effect produced by the difference of liquids. In this state of uncertainty the Royal Society adopted the most prudent plan that could be thought of; by giving its sanction to a thermometer to serve as a standard for the construction of those which philosophers should make use of. After this some men of genius endeavoured to establish fixed principles for the making of this instrument. Sir Isaac Newton led the way, but the utility of his first attempts was not sufficiently attended to. Fahrenheit and Reaumur then laboured with great care to settle this point, and we are much indebted to their inquiries. But Fahrenheit's principles were soon rejected, as being too uncertain, though his scale was preserved; and Mr. de Reaumur's, though in appearance admitted for a longer continuance, were in fact so indeterminate,
that,

that, without perceiving it, a deviation from 80 to 104, was made in the space between the two fundamental points of his thermometer.

65. If in the same manner we trace the origin of all instruments designed for nice mensuration, we should find that have all been defective at first, and gradually brought to perfection, when men of genius have thought them worthy of their attention. Thus from the first watch, which depended entirely upon the unequal and uncertain action of a spring, a succession of attempts has produced Mr. Harrison's valuable time-keeper; and from the first balances, which were either too heavy, or too light, we have attained to those scales of Mr. Matthey * as easily turned as they are accurate. What however is still more astonishing is, that, notwithstanding the importance of having fixt measures for the dimensions of bodies, we have not as yet used any in practice, but such as must be modeled immediately from others.

It is true, that in the construction of an Hygrometer, I was assisted by the general notions of regularity, gathered from the construction of other measures of the same kind; and of course my hygrometer is much forwarder in this respect than the thermometer, for instance, was in its origin. I therefore only compare the difficulties peculiar to the hy-

* An excellent mechanic, whose death is a loss to a king who knows the value of men of merit. He was a native of Vale-Orbe, in the Pays de Vaud, and in the service of his Sardinian Majesty; and has written a *Treatise on Balances*, which serves as a law to all the scale makers in the dominions of that prince.

grometer, to those that first occurred in all measures of physical causes; and I think that as the latter have been surmounted, we should not despair of conquering the former. In a word, it is certain that all our inventions only approach towards perfection by degrees, without ever attaining to it entirely; and for this very reason, we have a right to expect they will always be drawing nearer and nearer towards it.

Upon these notions chiefly, I have raised my hopes, either that my hygrometer will in time become more perfect, or at least that it may excite new ideas, which, will at length, though perhaps by some other road, lead us to a true measure of the humor. As the hope of attaining an end, is one of the most powerful assistants towards really arriving at it, I flatter myself at least that I sh^d have given birth to a reasonable one upon this subject.

First Views to improve the HYGROMETER.

66. The idea I entertain, that it is necessary a number of attentive men should concur, to improve the human inventions; has induced me first to mention the general reasons I had, for hoping that the hygrometer would be perfected. I shall now proceed to give some particular reasons on which this hope is founded, and which are collected from the remarks I have already made upon my instrument, during the little time I have had to observe it.

The first, and one of the most important of these remarks, is, that the ivory pipe belonging to that hygrometer which is always the highest upon its
scale,

scale, happens at the same time to be the thinnest of them all. What connexion there may be between these two circumstances, must be determined by experience. But in the mean time it appears to me that if the fibres of the ivory are interwoven with each other; they will make so much the less resistance either to the being separated or brought closer to each other, in proportion as the bundles of these fibres have a less degree of thickness. Whether this remark is of consequence, or not, we shall at least run no risk in making these ivory pipes always exactly of the same thickness. This indeed was my intention in those which I have made, but unfortunately I thought I should have been able to turn them upon cylinders of hard wood; and found too late, that no accuracy could be expected from this method. It was to remedy this inconvenience, that, in speaking of the manner of turning this piece, I have recommended brass cylinders (31).

67. The same precaution is likewise necessary to be taken, that we may be certain of giving to every pipe an equal degree of thickness throughout the whole of its circumference: a circumstance no less essential than the former; since I have observed in those of my hygrometers whose pipes have not an equal thickness, that they bend, more or less according to the degree of humor to which they are exposed.

This is probably the principal cause why these instruments do not always preserve the same proportions to each other (58). For the pipes not bending according to the same law, there must be an irregular

regular change in their capacity, and consequently in the height of the mercury in the tubes. The differences of this kind which I have had occasion to observe, are not indeed very considerable; but, however trifling the cause of an imperfection may be, it is still useful to remove it; were it only to assist us in discovering causes of greater imperfections, by making their effects the more evident.

68. But to make the ivory pipes keep straight, we must attend to a circumstance still more important; which is, that the texture of the ivory be the same in the whole circumference of the pipe. There is a sensible difference in the organization of the external, middle, and internal parts of the same elephant's tooth: nor is it impossible that, besides this difference in the nature, and visible arrangement of its fibres, there may be another arising from their degree of tension; so that some fibres may be more disposed than others to relaxation, after the tooth has been cut to pieces. Suppose then that any of these differences should exist in a pipe, that is, if one of its sides should be more porous, or of a weaker texture, than the other; or if its fibres should be more disposed to relaxation; this pipe will take a bend, either for a constancy, or for a time; and the hygrometers in which it is used, will not of course agree with the others. We must therefore endeavour to make these pipes with a part of the tooth that is homogeneous: that which I believe to be most so, within a certain extent, and which for that reason I have advised, is, the part which is between the center and the surface, and at some inches distance from the apex of the tooth (30).

69. There

69. There is another reason why this different organization of the different parts of the elephant's tooth makes it necessary to determine exactly the parts that are to be made use of in hygrometers. Without this precaution it might happen that the pipes, which ought to be similar in every respect, might be made of substances that really differed in their dilatibility and sensibility; that is, of substances which the humor might affect more or less strongly, or more or less quickly. This consideration will perhaps oblige us to determine both the size of the tooth, and the distance at which the piece ought to be cut off from its apex: for the organization may with equal probability vary in teeth of different thickness, and from the apex to the base; as it does in the breadth of the same tooth. I was not sufficiently certain of the success of my instrument, to take all these precautions when I first set about it, but at present I believe them to be important.

70. There is still another precaution, which indeed I thought necessary from the beginning, but which I could not manage as I wished for want of proper tools; that is, to perforate the ivory pipe exactly in the direction of its fibres. For let the channel have ever so small a degree of obliquity with respect to this direction of the fibres, these fibres will necessarily be cut in different places; which weakening the pipe where it happens, neither its dilatations nor its contractions can of course be regular.

71. I own here are a great number of precautions; but they will not surprize true philosophers. They are accustomed to observe the operations of

nature closely; and know that the regularity of her proceedings is connected with a foresight which is limited to us, by nothing but the limits of our abilities in tracing it; and consequently, when art attempts to imitate nature, it can only succeed in as much as it is attentive to imitate her care.

72. I believe that the hygrometer may farther acquire a perfection of the same kind as that which, in conformity to an idea of my worthy friend Mr. Le Sage, I have given to the thermometer; that is, that we may make its degrees correspond with equal differences in the humor; as I have made those of the thermometer correspond with equal degrees of heat. The way in which I think this might be done, would be to suspend near one of the hygrometers, in a proper vessel which should be placed in one of the scales of a balance that turns very easily, some substance remarkably greedy of the humor; the augmentations or diminutions of weight in which substance, might be compared with the going of the hygrometer, first in the same, and afterwards in different degrees of heat. I hope too, that by a frequent repetition of these observations, at times when the variations of the humor are more or less sudden, we shall at last succeed in correcting the errors that may attend them, from the loss of its own matter the substance made use of may probably suffer by evaporation.

73. These are not the only remarks I have made upon my instrument, but I did not care to mention any but such as have appeared to me most certain. The others are uncertain, and require longer observations. I shall only add therefore, that it will still

be necessary to make fresh experiments, in order to determine the length of time that the ivory pipes ought to remain in water, and how long they must afterwards be exposed to the vicissitudes of the air (28), or in general to what preparation they must be submitted, in order to acquire a lasting degree of consistence before they are made use of. For this purpose it will also be expedient to compare hygrometers recently made, with older ones, both to ascertain whether they have undergone any alterations, and in what degree. I likewise am of opinion, that when we wish to fix the point of extreme humidity, we must be very careful not to make use of any ice but what is very clean, as well internally as externally; lest any dust should stick to the ivory pipes, which might hinder the water from penetrating into the pores: this is what I thought of myself too late. I do not know whether for the same reason it would not be right to wash these tubes with spirits of wine before we put them into the water, to remove any greasy scurf they may have gathered by handling; and afterwards to repeat this at times, in order to carry off any little deposit of various kinds, which may in course of time have been left upon them by the air. Moreover it will be right to inquire whether there is not a difference between the effects of the heat upon the ivory of the hygrometers, and upon the glass of the thermometer, sensible enough to be attended to, in correcting the effects of this cause upon the hygrometer.

74. Having already discovered so many causes, more or less probable, of the differences I observed

in my hygrometers, I think it reasonable to hope that this instrument will receive a sensible degree of perfection on a second trial ; and that in time it will be brought to a sufficient degree of accuracy. It is true there are some difficulties in the way of this : but have we not sufficient motives for endeavouring to overcome them ? The air we breathe, and that which surrounds us ; the places we inhabit, and those which serve either to enclose or to preserve so many different bodies intended for our several occasions, are all of them more or less filled with that substance, differently modified, to which I have given the name of *humor*. It also produces very sensible effects in them ; some of which very properly excite our curiosity, -others may be turned to our advantage, and many of them essentially affect our health. It is therefore of great consequence to natural philosophy in general, and to œconomy, and medicine, in particular, that we should obtain a measure by which we may, with some degree of certainty, estimate the local and actual qualities of this substance, and by this means foresee its effects ; which for the generality we only become acquainted with after they are produced. These sciences are not perhaps less concerned that we should discover the nature itself of this agent, and the different manners in which it operates : the knowledge of which may enable us to avail ourselves of reason in the investigation of certain effects, which, without such helps, might escape our observation. As these are the several uses of an exact hygrometer, we may readily perceive how many new tracks such an instrument may open to us, in our investigations of nature, which however

we shall not owe to one man alone, but to the joint labours of several.

Account of some of the first Phænomena of the HUMOR observed with the HYGROMETER.

75. Though my first advances in this new track of observation are as yet very uncertain, yet I will not omit giving some account of them. They will at least serve to give some idea of the going of the instrument, as well as of the nature of the agent by which it is governed.

The first observation I attempted of this kind was with a view to one of the objects which made me desirous of having an hygrometer. These objects are all comprized in a general system concerning vapours, which I have given in my work upon the *Modifications of the Atmosphere*. I shall therefore only mention here one particular consequence of that system, which it was my immediate point to verify; namely, that a certain augmentation of heat, we always perceive at every season upon the approach of rain, is owing to a more than ordinary quantity of vapour; and that, on the contrary, it is to their diminished quantity that the lesser heat of the upper parts of the atmosphere is in great measure to be ascribed.

76. This latter consequence was supported by an accidental observation I made in September, 1770, upon a mountain of the Faucigny, at the height of 1560 toises above the level of the sea. An iron ferule, which served to unite the ends of a cleft stick, and which had been fixed on the stick with a
hammer

hammer upon the plain, in fine weather, came off of itself upon the top of the mountain. When this happened, the thermometer, which I called common, though exposed to the sun, was only three degrees above 0; while on the plain it was at 18 in the shade. This phenomenon, joined with several others I observed at the same time, confirmed me in my opinion, that one of the reasons why the upper parts of the atmosphere have less heat than the lower, is, that they contain less humor.

77. With this notion, it became a very interesting inquiry to know the different degrees of humidity habitual to the different heights of the atmosphere. Of course therefore this was the first observation I thought of, as soon as I had added an hygrometer to the other instruments contained within the box of my portable barometer. I undertook therefore to ascend Buet (the name of that high mountain) a second time. My companions in this expedition were Mr. Dentan, a very intelligent young philosopher, and my brother, who, having assisted me in all undertakings of any difficulty, had been a witness of that fact which was the object of my present researches.

78. At our setting out, on the 29th of last August, the hygrometer was at 86 in my apartment, and the barometer at 27 p. 1 line. We were in hopes of fine weather, because it is generally fair in this country when the barometer at Geneva is above 27 French inches. Soon after we set out, we began to perceive that the power of the sun was greater than might have been expected for the season. From this circumstance I concluded that the barometer must fall;

fall, and in fact we found it lower at every place in our way, where we had before observed it in fine weather. The sky, notwithstanding, was still clear, and continued so the next day, when we began to ascend the mountain, about two o'clock in the afternoon, in order to pass the night in the highest cottages, that we might have more time to gain the summit the next day.

79. Before we left Sixt (an abbey at the foot of the mountain), I exposed the hygrometer in open air, and in the shade it stood at 94. The thermometer at the same time was at 19 in the shade, and at 24 in the sun. At five o'clock we reached a place above 300 toises above the abbey; commanded on all sides by mountains, and on that account called *Les Fonds* (or The Bottoms). Here we observed the thermometer and hygrometer. The former, when exposed to the sun, stood at $15\frac{1}{4}$, and the latter rose to 96 in the shade. We observed them again in the same manner about half an hour after six, in a place that was pretty open, and higher by 160 toises than the former. The thermometer stood at 15, and the hygrometer at 106. It wanted but a quarter of nine, when we came to the cottages where we were to pass the night; though they were not above 30 toises higher than the place we stopped at last. The higher we went, the clearer the sky appeared; in so much that, notwithstanding the usual augmentation of humor in the air after sunset, when the sky is not clouded, upon exposing the instrument to the air, about $\frac{1}{4}$ after ten at night, we found the hygrometer at 123, and the thermometer at $13\frac{1}{4}$. They both fell in the night, and on our setting

letting out the next morning, the former had got down to 109, and the latter to 12.

80. In the two last mentioned observations the hygrometer had been exposed long enough to the open air, to conform itself to the degree of humor prevalent in the place; but we had not time for the observations I was most desirous to make with accuracy. The hygrometer being usually shut up in the box of my barometer, it would have been necessary to have left that open some time, in order that it should adapt itself to the state of the air, and we could allow but a very short time for these observations.

81. The first of them was made at nine in the morning, at the height of about 1000 toises above the plain. The sky appeared clear over head, but the plain was darkened with vapours. The thermometer in the sun stood at 13 $\frac{1}{2}$, and the hygrometer rose to 115 in the shade.

82. It was two in the afternoon when we reached the top of the mountain, which is always covered with an enormous mass of ice and snow. We found there a very strong south wind, which is the warmest wind in our plains: besides this, we were nearly at the hottest time of the day; and yet the thermometer, upon being exposed to the sun, shewed only 6. The wind, and the coldness of this region, obliged us to quit the summit in a quarter of an hour, during which the hygrometer had risen only to 119; but we judged that it was not yet stationary.

83. In this short time we experienced a new effect of the diminished humidity of the air, which surprised us all three very much. We found our skin withered

withered and pale, so that both to the sight and to the touch, it resembled much a dry and shriveled bladder. Notwithstanding this we were sensible of no other inconvenience but what arose from the wind and the cold : the action of the lungs and the functions of all the other parts of the body were perfectly free, though the barometer was only at 19 inches, 6 lines and a half.

84. We quitted the summit at about a quarter after two, to shelter ourselves from the wind behind some rocks, which were nearly 50 toises lower. Here we stayed about an hour. During this time the hygrometer, exposed to the air, but always in the shade, rose by imperceptible degrees to $132\frac{1}{4}$. It would probably have risen higher, had not we been obliged to quit this place, where the clouds began to gather, in order to reach the cottages before night. It was indeed already too late before we thought of retiring ; for we were overtaken by the night, and a thunder storm, at a sufficient distance from our hut to expose us to the greatest danger of being lost, notwithstanding our guides, but for the assistance of two women, whose humanity deserves the highest commendations. These women, who lived in our cottages ; being apprized of our distress by our cries, notwithstanding the storm, and the scarcity of wood in these places, came out to kindle a great fire at the foot of the rocks on which we were wandering amidst the precipices, in total darkness ; and sometimes with great difficulty keeping the fire alive, sometimes advancing towards us with fire-brands till the wind and rain extinguished them, and endeavoured, with the most unaffected concern, to point out to us the

path we ought to keep. At last, animated by the courage of these women, directed partly by their light and partly by their cries, we at length reached the cottage, much more affected with the humanity of these good people, than hurt with the dangers and fatigues we had undergone.

85. The storm lasted a great part of the night, and it rained almost without intermission. Notwithstanding this, the hygrometer, when exposed to the air the next morning, stood at 105, and the thermometer at 10. As we were uncertain how long the rain would continue, we set out at eight in the morning on our way down. The rain hardly ceased the whole morning, and was sometimes accompanied with hail; it still continued raining when we arrived at the abbey about noon, notwithstanding the hygrometer stood there at 99, that is to say, five degrees higher than when we set out; but the barometer, which had fallen the two preceding days, was now beginning to rise; the thermometer was at 14.

86. We learnt at Sixt, that, at the very time we were driven from the summit of the mountain by the disagreeable coldness of the air, they had felt an excessive degree of heat, and likewise that the storm had been very violent in the night. This storm, as we found two days after at Geneva, had extended itself all over the plain. We found likewise, from the observations that had been made there in our absence, that a thermometer exposed to the north, consequently out of the sun, had been at $23\frac{1}{2}$, at the very time that ours, at the top of the mountain and in the sun, had been only at 6.

87. As,

87. As, in mentioning the particular purport of the foregoing observations, I have not explained my system concerning vapours, I shall not here stop to draw the consequences that may be deduced from them in favour of this system. Indeed, to say the truth, I think them too few and too imperfect to conclude any thing from them as yet. I have only related them, as I declared at first, to give a general notion, both of the going of my hygrometer, and of the inquiries that may be pursued with its assistance. It is with the same intention that I proceed to relate some observations of another kind.

88. Some accidental observations had made me suspect that the immediate action of the sun upon my hygrometer produced a drying, which might not be wholly occasioned by the real state of the air with respect to the humor, but might depend in some measure upon some singular property of the solar rays, which we see produce effects upon some bodies, not immediately to be accounted for by the ordinary laws of heat. This first remark induced me, as I have taken care to mention, always to observe the hygrometer in the shade upon the mountains of Sixt. At my return, I determined to examine more accurately whether my conjecture in this respect had any foundation.

89. The first thought that occurred to me for this purpose was, to observe two hygrometers at the same time, one in the shade and the other in the sun, very near each other, that the same air might circulate freely round them. The air of the country having appeared to me more proper for this observation than

A TABLE of OBSERVATIONS made on the 13th of September, on two HYGROMETERS, the one in the Shade, and the other in the Sun, each of them accompanied with a THERMOMETER.

	Hour of the Day	Therm. in the Shade.	Hygrom. in the Shade.	Hygrom. in the Sun.	Ther. in the Sun.
The Bar. at 27 Fr. inch. 1 line. The sun did not shine yet on that part of the garden.	7	8	29	36 $\frac{1}{2}$	8
The sun has now shone for $\frac{1}{2}$ of an hour on the Hygr. and Therm. which are to stand exposed to it.	7 $\frac{1}{2}$	11 $\frac{1}{4}$	36 $\frac{1}{2}$	66 $\frac{1}{2}$	12
	8	12 $\frac{1}{4}$	43 $\frac{1}{2}$	82	12 $\frac{1}{2}$
	9	13	67	102	13 $\frac{1}{2}$
	10	14 $\frac{1}{2}$	76 $\frac{1}{2}$	109	15 $\frac{1}{2}$
	11	15 $\frac{1}{2}$	87 $\frac{1}{2}$	116	16 $\frac{1}{4}$
	Noon	15 $\frac{1}{2}$	96 $\frac{1}{2}$	120 $\frac{1}{2}$	17 $\frac{1}{4}$
	1	16 $\frac{1}{2}$	103	126	18
The vapours condensing in the air weaken the action of the sun.	2	16 $\frac{1}{4}$	103	125	17 $\frac{1}{4}$
Barom. 27. inch. A South wind be- gins to blow.	3	16 $\frac{3}{4}$	102 $\frac{1}{2}$	123	17 $\frac{1}{2}$
The clouds rise.	4	15 $\frac{1}{2}$	107	133	16
The clouds meet, and the sunshine withdrawn.	5	13 $\frac{1}{2}$	88 $\frac{1}{2}$	106	13 $\frac{1}{2}$
The sun is set, and the weather quite overcast.	6	12	64 $\frac{1}{2}$	81	12
Barom. 26 inches 11 lines. . .	7	11 $\frac{1}{4}$	50	65	11 $\frac{1}{4}$
	8	11	37	50	11
	9	10 $\frac{1}{2}$	31	41	10 $\frac{1}{4}$
The clouds break, and the dew be- gins to appear on the plants.	10	10 $\frac{1}{2}$	24	35	10 $\frac{1}{2}$
	11	10	20 $\frac{1}{2}$	26 $\frac{1}{2}$	10
The clouds meet again. . . .	Mid.	10 $\frac{1}{2}$	24 $\frac{1}{2}$	28 $\frac{1}{2}$	10 $\frac{1}{2}$
	1	11 $\frac{1}{4}$	23	27	11 $\frac{1}{4}$
It begins to rain.	2	11 $\frac{1}{2}$	27	22	11 $\frac{1}{2}$

92. The first circumstance in these observations that deserves to be noticed, is the difference in the sinking of the two hygrometers when they were exposed to the air, before the sun shone in the garden. They both of them fell considerably, but one of the two 7 degrees and a half less than the other. One of the causes of this disparity is probably in the instruments themselves, and is owing to their being differently affected by the action of the humor. There is a difference of the same kind observable in the thermometers, which are likewise more or less sensible to the impressions of the heat even when the bulk of their liquid is the same; that is to say, they are acted upon more or less quickly by the degree of heat which surrounds them, according to the thickness, or even according to the nature of the glass of which the ball is made. Consequently it is possible that the different thickness or porosity of the ivory may have had some influence on the going of the hygrometer in this observation (66 and 69).

93. But these differences in the ivory pipes must produce a much greater difference in the sensibility of the hygrometers, than those of the glass balls can produce in the thermometers; because it is much more difficult for the humor to penetrate the ivory, than for the heat to get through the glass. So that any increase of the obstacles retards the introduction of the humor, much more than that of the heat; and consequently the difference of sensibility must be more difficult to be prevented in the hygrometers, than it is in the thermometers.

This slowness of the humor in pervading the bodies into which it insinuates itself, makes it a desirable

firable circumstance, that the ivory pipe of the hygrometer should be the thinnest possible; in order that it might be more readily affected. This I had foreseen, before I had learnt it from experience; but I was afraid of its being attended with still greater inconveniences than that it was intended to remedy; from the action of the mercury against pipes whose sides would be thinner. However, this might be tried. In the mean time, I fancy that, for observations in which it is absolutely necessary that the instrument should easily be affected, lesser hygrometers might be made, whose tubes containing a less quantity of mercury, would resist the action of it, though with a less degree of thickness. (Perhaps it would not be impossible to use tubes made of some very thin quills.) I cannot yet ascertain whether these little hygrometers could be graduated by themselves, or whether they must be compared with those of which I have given the dimensions; this we shall learn from experience.

94. The difference there is between the heat and the discrete humor in the power of diffusing itself, occasions in another respect a considerable difference in the goings of the thermometer and hygrometer. The heat is brought into a state of equilibrium much sooner and with much greater certainty than the humor. Two thermometers accurately constructed and fixed near each other, in a place where the heat does not change very suddenly, always agree together. This is not the case with two hygrometers: they seldom agree, that is, they seldom preserve the same conformity to each other, when there is the least variation in the humor: at some times their difference increases, at others it diminishes; this can
only

only arise from a difference in the cause that acts upon them.

95. We may form our ideas of the manner in which the invisible humor distributes itself; from that in which all kinds of visible vapours are diffused. We see them separate, re-unite, fly off from certain places, rush into others, and in short yield to every impression of the air. The motion peculiar to their own particles, which I look upon as the cause of their elasticity *, is not sufficiently rapid, and the vapours themselves are too thick to overcome always the contrary motion of the air. This, I believe, is what constitutes the chief difference between vapours, and the igneous fluid, as far as relates to the power of putting themselves into a state of equilibrium in the air, which is moving. The current of air carried towards a chimney which has fire in it, frees the room from smoke, and is but a very slight impediment to the diffusion of the heat through it.

96. Though the invisible vapours by reason of their excessive thinness are more capable of being put in equilibrium in the air than the visible ones, they are very far from having this property in as great a degree as the heat. Which leads me to think, that part of the difference observed between my hygrometers, even before sunrise, may have been owing to the unequal distribution of the humor, though the two instruments were only at the distance of a foot from each other, without the interposition of any solid body.

* The system I adopted on that point may be found in my work upon the *Modifications of the atmosphere*.

97. I shall not attribute intirely to the same cause, the great difference observed between my hygrometers, when one was exposed to the sun, while the other stood in the shade. The immediate action of the solar rays, or of the luminous heat, produces a variety of effects, which, as I have said before, do not appear to follow the same laws as those of dark heat. And if I may be allowed to propose a conjecture upon this particular point, before fuller experiments have been made, it should seem, that the immediate action of the solar rays must occasion a greater evaporation than what is produced by dark heat, even when they hold the thermometer at the same height. But let the cause be as it will, we see by this experiment, that in a section of air about a foot wide, through which the solar rays did not immediately pass, the action of the humor upon the hygrometer was 23 degrees greater than in the place round about; though that of the heat upon the thermometers was only a degree and a quarter less; which leads us to conceive how many apparently small causes may contribute to produce sensible differences in the distribution of the discrete humor.

98. Another use to be made of these observations is, to compare them with those that I have made in the mountains of Sixt; in order to form a better judgment of the proportion between the different degrees of humidity, in the superior and inferior parts of the atmosphere. My hygrometer, held in the shade upon the summit of Buet, rose to 132½, and was not yet stationary. This is pretty nearly the greatest degree of dryness observed in the hygrometer exposed to the sun in the garden; while the

the one that remained in the shade, the same upon which the observation at the mountain had been made, was not in fact higher than 103, though marked in the table of observations at 107 (91)

99. But the difference between the observations made upon the mountain of Sixt, and those I am speaking of, was still greater by much after sun-set. The 30th of August, at a quarter after ten at night, I observed the hygrometer without side the cottage upon the mountain, and found it at 123 (79); and on the 13th of September following, in the plain, it was not higher than 31 at 9, and 24 at 10 o'clock. The wind was south, and the height of the barometer upon the plain, pretty much the same during both the observations.

100. It is true that, notwithstanding the similarity of these circumstances, these observations cannot be directly compared, on account of the disagreement in some other circumstances. In the first place, the difference of fourteen days at this season of the year may have produced a sensible change in the state of the air. There was already, for instance, a considerable difference in the degrees of the thermometer; it was at 13 and $\frac{1}{2}$ when the observation was made on the mountain, and no higher than at 10 on the plain. Besides, at this time of night, there would always be an essential difference between the upper and lower parts of the atmosphere, even though in the day time they should have the same degree of humidity: for the vapours being condensed after sun-set, and thus producing a kind of dew, they must necessarily descend, and from this very cause be more abundant in the low grounds than

on the higher ones. I shall add, that though my hygrometer was exposed to the open air on the mountain, as it was in the plain, yet it was not so much insulated there, being tied to the box of my portable barometer: The difference observed, however, is so considerable, that, notwithstanding the concurrence of all these particular causes, I cannot but ascribe it in some measure to that general one which I have suspected, namely, that there is comparatively a less degree of humidity in the upper than in the lower parts of the atmosphere.

101. The observation of the 13th of September seems likewise to throw some light upon the phenomena of dew. We know that when the sky is cloudy, there is little or no dew, and it has likewise been observed from this very circumstance, that the air is not so much cooled after sun-set. The cause of these differences appears to me to be, that when there are no clouds in the air at sun-set, or when they are dispersed, the heat of the inferior air, and that which rises from the earth, dissipates itself into the superior regions, and then the vapours which are dispersed throughout the air condense and fall down again in dew; but when the clouds are continued, and thus separate the inferior from the superior air, they prevent this dissipation of the heat, and the vapours remain suspended. And if the sky grows cloudy some hours after the setting of the sun, and after the heat has sensibly diminished in the inferior air, it encreases again in it; because the heat, which continues to rise out of the earth, is accumulated in the inferior air. This appears in the observation I am speaking of. The clouds having been separated

rated for a while, at 10 o'clock there was some dew, and the hygrometer fell sensibly till eleven : but afterwards the clouds closing again, the heat encreased, and the humidity evidently diminished.

102. I take it for granted here, that the most common and most plentiful dew proceeds from the air, and not from the earth, as some philosophers have imagined. I should produce the proofs I have collected of this fact from a multitude of experiments, if it had not been done in an excellent paper, written by Professor le Roi, *On the elevation and suspension of water in the air* *. These phænomena of the dew become very interesting examined with the help of the hygrometer, and joined to observations of the degrees of saturation of the air with respect to water, which have been so ingeniously imagined, and begun by the author of this memoir. If this part of natural philosophy is ever cleared up, as I hope it will be, we shall be much indebted for it to the sagacity of this true philosopher.

103. I shall only mention one more observation I have endeavoured to make with my hygrometer, which ought not to be omitted, as it is connected with the principles upon which the instrument is constructed. It has likewise a reference to medicine, in as much as one of the objects of that science, in its inquiries to preserve our health, is to determine the effects of water at different degrees of heat upon our organs. Ivory being an animal substance, the effects produced upon it by water at different degrees

* Mem. de l'Ac. des Sc. de Paris, for the year 1751.

of heat, may assist us in discovering those which are produced upon our bodies from the same cause.

104. The point 0 of my hygrometer, as I have before observed (44), is that of the extreme humidity produced by melting ice. It was therefore of some importance to know what difference there would be in this point, when the hygrometer should be plunged into warmer water. This I endeavoured to find out; and the following is the result of my first inquiries.

105. The moment I took one of my hygrometers out of melting ice, I plunged it into water at the heat of 45 degrees of the thermometer that I have called common. It fell suddenly four of its degrees below the thread which marked its height in the melting ice, but immediately rose again, and in four minutes reached 8 degrees and a half above the same thread. Deducting $22\frac{1}{2}$ from the height for the dilatation of the mercury (48), there will remain 14. Consequently the water warmed at 45 degrees of the common thermometer, really made the hygrometer sink 14 degrees below 0.

106. Half an hour after this, the water being at 38 degrees, I found the hygrometer no higher than $6\frac{1}{2}$, that is to say, $6\frac{1}{2} - \frac{38}{2} = -12\frac{1}{2}$. Consequently the true point of humidity indicated by the hygrometer was $12\frac{1}{2}$ below 0. Lastly, the heat of the water being reduced to 28 degrees the hygrometer was at $3 - \frac{28}{2} = -11$. I was then obliged to put an end to the experiment, which I have not been able to take up again since, for want of leisure. But
 1 what

what has already been observed is sufficient to shew us that the warmer the water is, the more it dilates the ivory (though we saw that the mercury rose in the hygrometer after having sunk for a moment). From hence, I fancy, may be drawn this general consequence, already indeed foreseen, namely, that in *an equal acting quantity*, the warmer the humor is, the more it separates the particles of those bodies which it pervades.

107. I say, in an *equal acting quantity*; and this is one of the objects which will probably furnish us with a variety of most useful knowledge, at the same time that it is most likely to give the greatest exercise to the genius and attention of natural philosophers. The forementioned experiment proves, that the warmer the water is, the more it dilates the ivory pipe of the hygrometer, and the same thing I make no doubt happens with the discrete humor.

On the other hand, the evaporation being certainly greater in summer than in winter, there must of course be more vapours in the air in the first of these seasons than in the latter. These then, as it appears, are the two circumstances most likely to make the hygrometer fall in summer; a greater degree of humor in the air, and an encrease of heat. And yet I have already experienced that the mean height of the hygrometer is greater in summer than in the other seasons. I found my first hygrometer, which was made in winter, too short in the summer; but it would be of a sufficient length now that we are in autumn. The mean height of the four new ones is already (the beginning of November)

ber) 17 degrees less than it was in the months of August and September.

108. I hope this paradox will be explained, and that the principles which may clear it up will draw useful consequences along with them. Those philosophers who look upon evaporation as a dissolution of water by air in the manner of menstrua, that is, by affinity, will easily apply their principle to the solution of part of these phænomena. The dissolution is greater when the menstruum is warmer, and consequently the air must keep a greater quantity of water in dissolution, and suffer a less part of it to be precipitated, in summer than in winter. I cannot but allow that this system is extremely specious, and that many phænomena are very happily explained by means of it. This is what Mr. le Roy has shewn us in the memoir I have already quoted; in which, without contending that air really acts as a menstruum with respect to water, he demonstrates, by a parallel very well kept up, that all the chemical expressions concerning dissolutions may with propriety be applied to describe the several phænomena he examines, relative to the elevation and suspension of water in air, as well as to its precipitation under different forms.

109. If it was not too common a practice, to conclude things from words, I should in fact think these chemical expressions very conveniently adapted to explain a number of these phænomena. But I have rejected them here, on account of this consideration; that when I took in a greater number of phænomena, I found them no longer accurate, any more than the general idea of the dissolution
tion

tion of water by air. I have given the reasons for this in my work, upon the modification of the atmosphere; and shall only repeat here, that these modifications of the humor appear to me almost intirely to be produced by the igneous fluid; and that if the air has any share in them, it is only as being an elastic fluid. The particles of these fluids, each according to its degree of power, strike, separate, and draw along with them those of the humor, and communicate to them the elasticity they possess; in the same manner as they do to the particles of all volatile, and likewise of all fixt substances which they corrode and decompose.

110. This system will not only furnish a solution of the paradox which engages our attention, but will, I believe, carry us much farther. The heat of the summer keeps the humor in very great agitation; and though there is more of the humor at this season than in winter, yet this heat will not allow it to continue either as long a time, or in as great a quantity, upon the bodies or in their pores. That is the reason why the hygrometer falls less. But we see at the same time, that the portion of the humor which does sojourn, and which I call the active part, has more power to dilatate the bodies, from the greater degree of motion impressed upon it by a greater heat. Consequently the dilatation of the bodies, from this cause, will be in a compound ratio of the quantity of humor, and of its active force, or of the heat. And if, for instance, we compare any summer's day, in which the hygrometer in open air is at the same degree as on any winter's day, the air on the summer's day will contain more humor than on the

winter's day; but there will be less of it will act upon the hygrometer; and yet as the active part will have more strength, the effect upon the whole will be the same. This is what appears to me, but I can not now enlarge any farther on this system. I have said enough to shew that the subject is very expensive, and deserves an attentive examination.

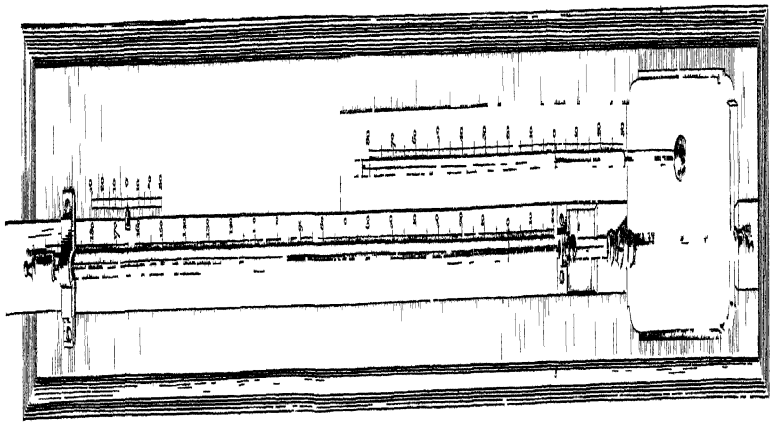
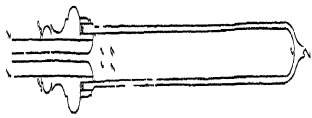


Fig. II.

Fig. I.



Apparatus.

XXXIX. *Of the electric Property of the Torpedo. In a Letter from John Walfh, Esq; F. R. S. to Benjamin Franklin, Esq; LL.D., F. R. S., Ac. R. Par. Soc. Ext., &c.*

Chesterfield-Street, July 1, 1773.

DEAR SIR,

Read July 1, 1773. **I** Am concerned that other engagements have prevented me from giving to the Royal Society, before their recess, a complete account of my experiments on the electricity of the Torpedo; a subject not only curious in itself, but opening a large field for interesting inquiry, both to the electrician in his walk of physics, and to all who consider, particularly or generally, the animal œconomy.

To supply the deficiency in the best manner I am now able, I will request the favour of you to lay before the Society my letter from la Rochelle, of the 12th July 1772, and such part of the letter I afterwards wrote from Paris, as relates to this subject. Loose and imperfect as these informations are, for they were never intended for the public eye, they are still the most authentic, and so far the most

satisfactory I can at present offer, since the notes I made of the experiments themselves remain nearly, I am sorry to say it, in that crude and bulky state in which you had the trouble to read them.

Letter from Mr. WALSH to Dr. FRANKLIN, dated
la Rochelle, 12th July 1772.

“ It is with particular satisfaction I make to you
“ my first communication, that the effect of the
“ Torpedo appears to be absolutely electrical; by
“ forming its circuit through the same conductors
“ with electricity, for instance metals, and water;
“ and by being intercepted by the same non-con-
“ ductors, for instance glass, and sealing-wax. I
“ will not at present trouble you with the detail of
“ our experiments, especially as we are daily ad-
“ vancing in them; but only observe, that we have
“ discovered the back and breast of the animal to be
“ in different states of electricity: I mean in par-
“ ticular the upper and lower surfaces of those two
“ assemblages of pliant cylinders, of which you
“ have seen engravings in Lorenzini *. By the
“ knowledge of this circumstance we have been
“ able to direct his shocks, though they were very
“ small, through a circuit of four persons, all feel-
“ ing them; likewise through a considerable length
“ of wire held by two insulated persons, one touching
“ his lower surface, and the other his upper. When

* Osservazioni intorno alle Torpedini di Stef. Lorenzini
1678.

Redi appears to be the first who remarked these singular parts
of the Torpedo in 1666. Franc. Redi, Exper. Nat.

“ the

“ the wire was exchanged for glass, or sealing-wax,
 “ no effect could be obtained: but as soon as it was
 “ resumed, the two persons became liable to the
 “ shock. These experiments have been varied many
 “ ways, and repeated times without number, and
 “ they all determined the choice of conductors
 “ to be the same in the Torpedo as in the Ley-
 “ den Phial. The sensations likewise, occasioned
 “ by the one and the other in the human frame, are
 “ precisely similar. Not only the shock, but the
 “ numbing sensation which the animal sometimes
 “ dispenses, expressed in French by the words *en-*
 “ *gourdissement* and *fourmillement*, may be exactly imi-
 “ tated with the Phial, by means of Lane’s Electro-
 “ meter; the regulating rod of which, to produce the
 “ latter effect, must be brought almost into contact
 “ with the prime conductor which joins the Phial.
 “ We have not yet perceived any spark to accompany
 “ the shock, nor the pith balls to be ever affected.
 “ Indeed all our trials have been on very feeble
 “ subjects, whose shock was seldom sensible beyond
 “ the touching finger: I remember but one, of at
 “ least two hundred, that I myself must have re-
 “ ceived, to have extended above the elbow. Per-
 “ haps the Isle of Ké, which we are about to visit,
 “ may furnish us with Torpedos fresher taken and of
 “ more vigour, by which a farther insight into these
 “ matters may be had. Our experiments have been
 “ chiefly in the air, where the animal was more
 “ open to our examination than in water. It is a
 “ singularity that the Torpedo, when insulated,
 “ should be able to give to us, insulated likewise,
 “ forty or fifty successive shocks from nearly the
 “ same

“ same part; and these with little, if any diminu-
 “ tion in their force: indeed they were all very
 “ minute. Each effort in the animal to give the
 “ shock is conveniently accompanied with a de-
 “ pression of his eyes, by which even his attempts
 “ to give it to non-conductors can be observed.
 “ The animal, with respect to the rest of his body,
 “ is in a great degree motionless, but not wholly so.
 “ You will please to acquaint Dr. Bancroft, of our
 “ having thus verified his suspicion concerning the
 “ Torpedo *, and make any other communication
 “ of this matter you may judge proper. Here I
 “ shall be glad to excite, as far as I am able, both
 “ electricians and naturalists, to push their inquiries
 “ concerning this extraordinary animal, whilst the
 “ summer affords them the opportunity.”

Extracts of a Letter from Mr. WALSH to Dr.
 FRANKLIN, dated Paris, 27th August, 1772.

——— “ I spent a complete week in my ex-
 “ periments at the Isle of Ré, and had there every
 “ convenience for prosecuting them to their extent,
 “ except that I was restrained by the jealousy of
 “ the government from making them where the
 “ animal was caught. At my return to la Ro-
 “ chelle, I communicated to the members of the
 “ Academy of that place, and to many of the prin-
 “ cipal inhabitants, all that I had observed con-
 “ cerning the Torpedo, in the intention of stirring

* Bancroft's Natural History of Guiana, p. 194.

“ up a spirit of inquiry, both as to its electricity and
 “ general œconomy.

“ ——— “ The vigour of the fresh taken Tor-
 “ pedos at the Isle of Ré, was not able to force the
 “ torpedinal fluid across the minutest tract of air;
 “ not from one link of a small chain, suspended
 “ freely, to another; not through an almost in-
 “ visible separation, made by the edge of a penknife,
 “ in a slip of tinfoil pasted on sealing-wax. The
 “ spark therefore (of course the attendant snapping
 “ noise) was denied to all our attempts to discover
 “ it, not only in day-light, but in complete dark-
 “ ness. I observed to you, in my last, the singu-
 “ larity of the Torpedo being able, when insulated,
 “ to give to an insulated person a great number of
 “ successive shocks: in this situation I have taken
 “ no less than fifty from him in the space of a
 “ minute and an half. All our experiments con-
 “ firmed, that his electricity was condensed, in
 “ the instant of its explosion, by a sudden energy
 “ of the animal; and as there was no gradual
 “ accumulation, nor retention of it, as in the case
 “ of charged glass, it is not at all surprizing that
 “ no signs of attraction or repulsion were perceived
 “ in the pith balls. In short, the effect of the Tor-
 “ pedo appears to arise from a compressed elastic
 “ fluid, restoring itself to its equilibrium in the same
 “ way, and by the same mediums, as the elastic fluid
 “ compressed in charged glass. The skin of the
 “ animal, bad conductor as it is, seems to be a
 “ better conductor of his electricity, than the
 “ thinnest plate of elastic air. Notwithstanding the
 “ weak spring of the torpedinal electricity, I was
 “ able

“ able, in the public exhibitions of my experiment,
 “ at la Rochelle, to convey it through a circuit,
 “ formed from one surface of the animal to the
 “ other, by two long brass wires, and four persons,
 “ which number, at times, was increased even to
 “ eight. The several persons were made to com-
 “ municate with each other, and the two outermost
 “ with the wires, by means of water contained in
 “ basins, properly disposed between them for the
 “ purpose; each person dipping his hands in the
 “ nearest basins, connectively with his neighbour on
 “ either side. —

“ The effect produced by the Torpedo, when
 “ in air, appeared, on many repeated experiments,
 “ to be about four times as strong as when in
 “ water.”

A clear and succinct narrative of what passed at one of the public exhibitions, alluded to in the last letter, appeared in the French Gazette of the 30th October 1772. As it came from a very respectable quarter, not less so from the private character of the gentleman, than from the public offices he held, I must desire leave of the Society to avail myself of such a testimony to the facts I have advanced, by giving a translation of that narrative.

“ ~~Extract~~ of a Letter from the Sieur SEIGNETTE,
 Mayor of la Rochelle, and second perpetual
 Secretary of the Academy of that City, to the
 publisher of the French Gazette.

“ In the Gazette of the 14th August, you men-
 “ tioned the discovery made by Mr. Walsh, Mem-
 “ ber

“ ber of the parliament of England, and of the
 “ Royal Society of London. The experiment, of
 “ which I am going to give you an account, was
 “ made in the presence of the Academy of this
 “ city. A live Torpedo was placed on a table.
 “ Round another table stood five persons insulated.
 “ Two brass wires, each thirteen feet long, were
 “ suspended to the ceiling by silken strings. One
 “ of these wires rested by one end on the wet
 “ napkin on which the fish lay; the other end
 “ was immersed in a basin full of water placed
 “ on the second table, on which stood four other
 “ basins likewise full of water. The first person
 “ put a finger of one hand in the basin in which
 “ the wire was immersed, and a finger of the
 “ other hand in the second basin. The second
 “ person put a finger of one hand in this last
 “ basin, and a finger of the other hand in the
 “ third; and so on successively, till the five per-
 “ sons communicated with one another by the
 “ water in the basins. In the last basin one end
 “ of the second wire was immersed; and with
 “ the other end Mr. Walsh touched the back of
 “ the Torpedo, when the five persons felt a com-
 “ motion which differed in nothing from that of
 “ the Leyden experiment, except in the degree of
 “ force. Mr. Walsh, who was not in the circle
 “ of conduction, received no shock. This expe-
 “ riment was repeated several times, even with
 “ eight persons; and always with the same suc-
 “ cess. The action of the Torpedo is commu-
 “ nicated by the same mediums as that of the
 “ electric fluid. The bodies which intercept the
 “ action

“ action of the one, intercept likewise the action
 “ of the other. The effects produced by the
 “ Torpedo resemble in every respect a weak elec-
 “ tricity.”

This exhibition of the electric powers of the Torpedo, before the Academy of La Rochelle, was at a meeting, held for the purpose in my apartments, on the 22d July 1772, and stands registered in the journals of the Academy.

The effect of the animal was, in these experiments, transmitted through as great an extent and variety of conductors as almost at any time we had been able to obtain it, and the experiments included, nearly, all the points, in which its analogy with the effect of the Leyden Phial had been observed. These points were stated to the gentlemen present, as were the circumstances in which the two effects appeared to vary. It was likewise represented to them, That our experiments had been, almost wholly with the animal in air: That its action in water was a capital desideratum: That indeed all as yet done was little more than opening the door to inquiry: That much remained to be examined by the Electrician as well as by the Anatomist: That as artificial electricity had thrown light on the natural operation of the Torpedo, this might in return, if well considered, throw light on artificial electricity, particularly in those respects in which they now seemed to differ: That for me, I was about to take leave of the animal, as nature had denied it to the British seas; and that the prosecution of these researches

rested in a particular manner with them, whose shores abounded with it.

The Torpedo, on this occasion, dispensed only the distinct, instantaneous stroke, so well known by the name of the electric shock. That protracted but lighter sensation, that Torpor or Numbness which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and shewn to be producible by a quick consecution of minute shocks. This, in the Torpedo, may perhaps be effected by the successive discharge of his numerous cylinders, in the nature of a running fire of musketry: the strong single shock may be his general volley. In the continued effect, as well as in the instantaneous, his eyes, usually prominent, are withdrawn into their sockets.

The same experiments, performed with the same Torpedos, were on the two succeeding days, repeated before numerous companies of the principal inhabitants of La Rochelle. Besides the pleasure of gratifying the curiosity of such as entertained any on the subject, and the desire I had, to excite a prosecution of the inquiry, I certainly, wished to give all possible notoriety to facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science. Great authorities had given a sanction to other solutions of the phenomena of the Torpedo; and even the Electrician might not readily listen to assertions, which seemed, in some respects, to combat the general principles of electricity. I had reason to

make such conclusions from different conversations I had held on the subject with eminent persons both at London and Paris. It is but justice to say, that of all in that class you gave me the greatest encouragement to look for success in this research, and even assisted me in forming hypotheses, how the Torpedo, supposed to be endued with electric properties, might use them in so conducting an element as water.

After generally recommending to others an examination of the electric powers of these animals when acting in water, I determined, before I took my final leave of them, to make some farther experiments myself with that particular view ; since, notwithstanding the familiarity in which we may be said to have lived with them for near a month, we had never detected them in the immediate exercise of their electric faculties against other fish, confined with them in the same water, either in the circumstance of attacking their prey, or defending themselves from annoyance : and yet that they possessed such a power, and exercised it in a state of liberty, could not be doubted.

A large Torpedo, very liberal of his shocks, being held with both hands by his electric organs above and below, was briskly plunged into water to the depth of a foot, and instantly raised an equal height into air ; and was thus continually plunged and raised, as quick as possible, for the space of a minute. In the instant his lower surface touched the water in his descent, he always gave a violent shock, and another still more violent in the instant of quitting the water in his ascent ;

ascent; both which shocks, but particularly the last, were accompanied with a writhing in his body, as if meant to force an escape: Besides these two shocks from the surface of the water, which may yet be considered as delivered in the air, he constantly gave at least two, when wholly in the air, and constantly one and sometimes two, when wholly in the water. The shocks in water appeared, as far as sensation could decide, not to have near a fourth of the force of those at the surface of the water, nor much more than a fourth of those intirely in air.

The shocks received in a certain time were not, on this occasion, counted by a watch, as they had been on a former, when fifty were delivered, in a minute and a half, by the animal in an insulated and unagitated state: But from the quickness, with which the immersions were made, it may be presumed there were full twenty of these in a minute; from whence the number of shocks, in that time, must have amounted to above an hundred. This experiment, therefore, while it discovered the comparative force between a shock in water and one in air, and between a shock delivered with greater exertion on the part of the animal and one with less, seemed to determine, that the charge of his organs with electricity was effected in an instant, as well as the discharge.

The Torpedo was then put into a flat basket, open at top, but secured by a net with wide meshes, and, in this confinement, was let down into the water, about a foot below the surface; being

being there touched, through the meshes, with only a single finger, on one of his electric organs, while the other hand was held, at a distance, in the water, he gave shocks, which were distinctly felt in both hands.

The circuit for the passage of the effect being contracted to the finger and thumb of one hand, applied above and below to a single organ, produced a shock, to our sensation, of twice the force of that in the larger circuit by the arms.

The Torpedo, still confined in the basket, being raised to within three inches of the surface of the water, was there touched with a short iron bolt, which was held, half above and half in the water, by one hand, while the other hand was dipped, as before, at a distance in the water; and strong shocks, felt in both hands, were thus obtained through the iron.

A wet hempen cord being fastened to the iron bolt, was held in the hand above water, while the bolt touched the Torpedo; and shocks were obtained through both those substances.

A less powerful Torpedo, suspended in a small net, being frequently dipped into water and raised again, gave, from the surface of the water, slight shocks through the net to the person holding it.

These experiments in water manifested, That bodies, immersed in that element, might be affected by immediate contact with the Torpedo; That the shorter the circuit in which the electricity

tricity moved, the greater would be the effect; And that the shock was communicable, from the animal in water, to persons in air, through some substances.

How far harpoons and nets, consisting of wood and hemp, could in like circumstances, as it has been frequently asserted, convey the effect, was not so particularly tried as to enable us to confirm it. I mention the omission in the hope that some one may be induced to determine the point by express trial.

We convinced ourselves, on former occasions, that the accurate Kæmpfer *, who so well describes the effect of the Torpedo, and happily compares it with lightning, was deceived in the circumstance, that it could be avoided by holding in the breath, which we found no more to prevent the shock of the Torpedo, when he was disposed to give it, than it would prevent the shock of the Leyden Phial.

Several persons, forming as many distinct circuits, can be affected by one stroke of the animal, as well as when joined in a single circuit. For instance, four persons, touching separately his upper and lower surfaces, were all affected; two persons likewise, after the electricity had passed through a wire into a basin of water, transmitted it from thence, in two distinct channels, as their sensation convinced them, into another basin of water, from whence it was conducted, probably in an united state, by a single wire. How

* Kæmpf. Amoen. Exot. 1712, p. 514.

much further the effect might be thus divided and subdivided into different channels, was not determined; but it was found to be proportionably weakened by multiplying these circuits, as it had been by extending the single circuit.

Something may be expected to be said of the parts of the animal immediately concerned in producing the electrical effect. The engraving, which accompanies this letter, while it shews the general figure of the Torpedo, gives an internal view of his electric organs. The Society will, besides, have a full anatomical description of these parts from the ingenious Mr. John Hunter, in a paper he has expressly written on the subject at my request. It would therefore be superfluous for me to say any thing either in regard to their situation or structure.

I have to observe, however, That in these double organs resides the electricity of the Torpedo, the rest of his body appearing to be no otherwise concerned in his electrical effect than as conducting it: That they are subject to the will of the animal; but whether, like other double parts so controllable, they are exercised, at times, singly as well as in concert, is difficult to be ascertained by experiment: That their upper and under surfaces are capable, from a state of equilibrium with respect to electricity, of being instantly thrown, by a meer energy, into an opposition of a *plus* and *minus* state, like that of the charged Phial: That, when they are thus charged, the upper surfaces of the two are in the same state of electricity; as are the under surfaces

surfaces of the two, though in a contrary to that of the upper; for no shock can be obtained by an insulated person touching both organs above, or both below: And that the production of the effect depends solely on an intercourse being made between the opposite surfaces of the organs, whether taken singly or jointly.

All the parts bordering on the organs act, more or less, as conductors, either through their substance or by their superficies. While an insulated person, placing two fingers on the same surface of one or both organs, cannot be affected; if he removes one of his fingers to any such contiguous part, he will be liable to a shock: but this shock will not be near, perhaps not half, so violent, as one taken immediately between the opposite surfaces of the organ; which shews the conduction to be very imperfect.

The parts, which conduct the best, are the two great lateral fins bounding the organs outwardly, and the space lying between the two organs inwardly. All below the double transverse cartilages scarcely conduct at all, unless when the fish is just taken out of water and is still wet, the mucus, with which he is lubricated, shewing itself, as it dries, to be of an insulating nature.

The organs themselves, when uncharged, appeared to be, not interiorly we might suppose, but rather exteriorly, conductors of a shock. An insulated person touching two Torpedos, lying near one another on a damp table, with fingers placed, one on the organ of one fish, and another

on the organ of the other, was sensible of shocks, sometimes delivered by one fish, and sometimes by the other, as might be discovered by the respective winking of their eyes. That the organs, uncharged, served some way or other as conductors, was confirmed with artificial electricity, in passing shocks by them; and in taking sparks from them, when electrified.

The electric effect was never perceived by us to be attended with any motion or alteration in the organs themselves, but was frequently accompanied with a little transient agitation along the cartilages which surround both organs: this is not discernible in the plump and turgid state of the animal, while he is fresh and vigorous; but as his force decays, from the relaxation of his muscles, his cartilages appear through the skin, and then the slight action along them is discovered.

May we not from all these premises conclude, that the effect of the Torpedo proceeds from a modification of the electric fluid? The Torpedo resembles the charged Phial in that characteristic point of a reciprocation between its two surfaces. Their effects are transmitted by the same mediums; than which there is not perhaps a surer criterion to determine the identity of subtle matter: They, besides, occasion the same impression on our nerves. Like effects have like causes. But it may be objected, that the effects of the Torpedo, and of the charged Phial, are not similar in all their circumstances; that the charged Phial occasions attractive or repulsive dispositions

in neighbouring bodies ; and that its discharge is obtained through a portion of air, and is accompanied with light and sound ; nothing of which occurs with respect to the Torpedo.

The inaction of the electricity of the animal in these particulars, whilst its elastic force is so great as to transmit the effect through an extensive circuit and in its course to communicate a shock, may be a new phænomenon, but is no ways repugnant to the laws of electricity ; for here too, the operations of the animal may be imitated by art.

The same quantity of electric matter, according as it is used in a dense or rare state, will produce the different consequences. For example, a small Phial, whose coated surface measures only six square inches, will, on being highly charged, contain a dense electricity capable of forcing a passage through an inch of air, and afford the phænomena of light, sound, attraction, and repulsion. But if the quantity condensed in this Phial, be made rare by communicating it to three large connected jars, whose coated surfaces shall form together an area 400 times larger than that of the Phial (I instance these jars because they are such as I use) ; it will, thus dilated, yield all the negative phænomena, if I may so call them, of the Torpedo ; it will not now pass the hundredth part of that inch of air, which in its condensed state it sprung through with ease ; it will now refuse the minute intersection in the strip of tinfoil ; the spark and its attendant sound, even the attraction or repulsion of light bodies, will now

be wanting; nor will a point brought however near, if not in contact, be able to draw off the charge: and yet, with this diminished elasticity, the electric matter will, to effect its equilibrium, instantly run through a considerable circuit of different conductors, perfectly continuous, and make us sensible of an impulse in its passage.

Let me here remark, that the sagacity of Mr. Cavendish in devising and his address in executing electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air.

But, after the discovery that a large area of rare electricity would imitate the effect of the Torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This, however, we know, that from infinite division of parts infinite surface may arise, and even our gross optics tell us, that those singular organs, so often mentioned, consist like our electric batteries of many vessels, call them cylinders or hexagonal prisms, whose superficies taken together furnish a considerable area.

I rejoice in addressing these communications to You. He, who predicted and shewed that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds an humbler bolt, silent and invisible: He, who analysed the electrified Phial, will hear with
 2 pleasure

pleasure that its laws prevail in animate Phials :
He, who by Reason became an electrician, will
hear with reverence of an instinctive electrician,
gifted in his birth with a wonderful apparatus,
and with the skill to use it.

However I may respect your talents as an electrician, it is certainly for knowledge of more general import, that I am impressed with that high esteem, with which I remain,

Dear Sir,

Your affectionate

and obedient servant,

John Walfsh.

EXPLANATION of the PLATE

OF THE

Male and Female Torpedo, or Electric Ray.

T A B. XIX.

F I G. I.

A view of the under surface of the female.

- a.* An exposure, on flaying off the skin, of the right electric organ, which consists of white pliant columns, in a close and for the most part hexagonal arrangement, giving the general appearance of a honey-comb in miniature. These columns have been sometimes denominated cylinders ; but, having no interstices, they are all angular, and chiefly six-cornered.
- b.* The skin which covered the organ, shewing on its inward side an hexagonal net work.
- c.* The nostrils in the form of a crescent.
- d.* The mouth in a crescent contrary to that of the nostrils, furnished with several rows of very small hooked teeth.

e. The

- e.* The branchial apertures, five on each side.
- f.* The place of the heart.
- g. g. g.* The place of the two anterior transverse cartilages, which, passing one above and the other below the spine, support the diaphragm, and uniting towards their extremities, form on either side a kind of clavicle and scapula.
- h. h.* The outward margin of the great lateral fin.
- i. i.* Its inner margin, confining with the electric organ.
- k.* The articulation of the great lateral fin with the scapula.
- l.* The abdomen.
- m. m. m.* The place of the posterior transverse cartilage which is single, united with the spine, and supports on each side the smaller lateral fins.
- n. n.* } The two smaller lateral fins.
- n. n.* }
- o.* The anus.
- p.* The fin of the tail.

FIG. II.

A view of the upper surface of the female.

- a. a.* An exposure of the upper part of the right electric organ.
- b.* The skin which covered the organ.
- c.* The eyes, prominent and looking horizontally outwards, but capable of being occasionally withdrawn into their sockets.
- d.* Two circular apertures communicating with the mouth, and furnished each with a membrane, which in air, as well as in water, plays regularly backwards and forwards across the aperture in the office of inspiration.
- e.* The place of the right branchia.
- f.* The two fins of the back.
- g. g.* The place of the anterior transverse cartilages.

FIG. III.

A view of the under surface of the male, whose size, as here represented, is, in general, smaller than that of the female.

- a. a.* Two appendices, distinguishing the male species.

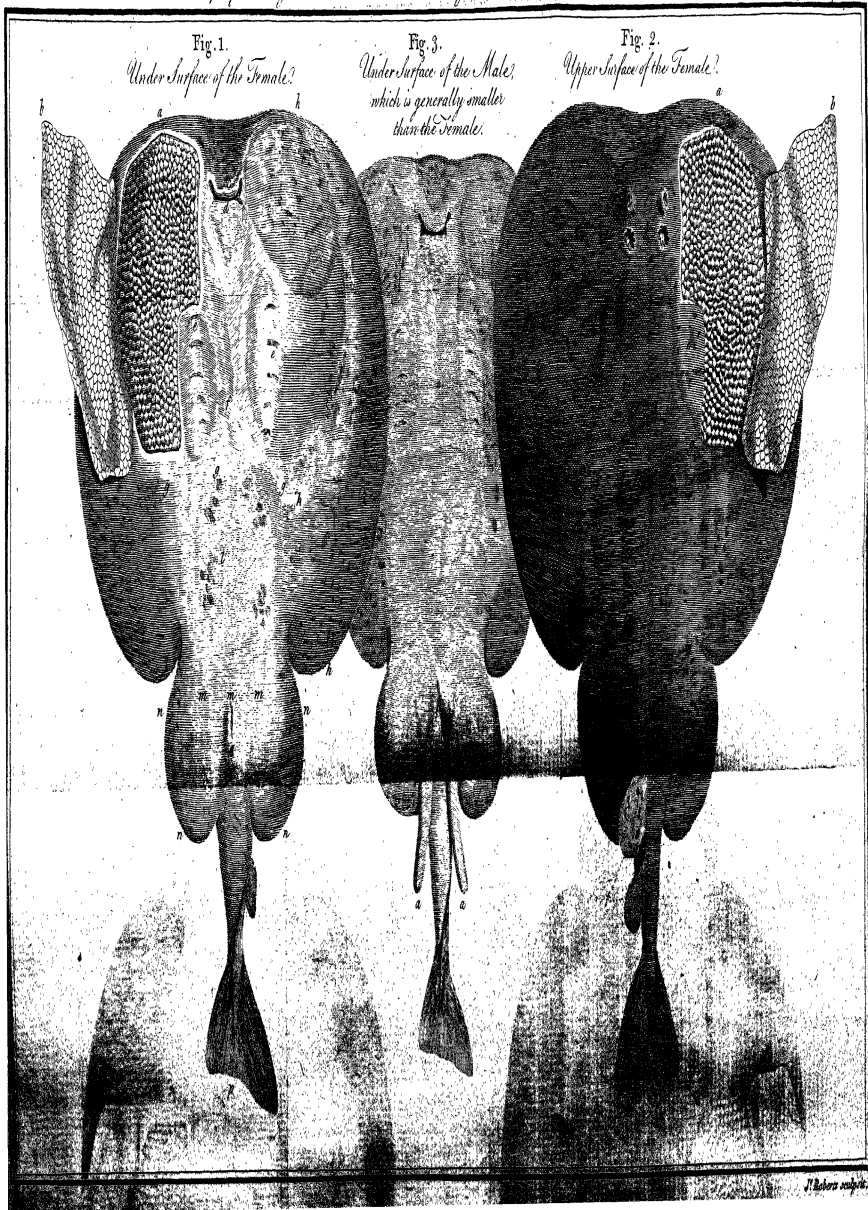
The Male & Female TORPEDO or ELECTRIC RAY,
frequenting the Sea Shores in the Neighbourhood of LA ROCHELLE.

Philos Trans 178 LXXI, Tab. III, p. 488.

Fig. 1.
Under Surface of the Female?

Fig. 3.
*Under Surface of the Male?
 which is generally smaller
 than the Female.*

Fig. 2.
Upper Surface of the Female?



XL. Anatomical Observations on the Torpedo. By John Hunter, F. R. S.

Read July 1, 1773. **I** WAS desired some time since, by Mr. Walsh, whose experiments at La Rochelle had determined the effect of the Torpedo to be electrical, to dissect and examine the peculiar organs by which that animal produces so extraordinary an effect. This I have done in several subjects furnished to me by that Gentleman.

I am now desired by him to lay before the Society, the observations I have made; and for the better understanding of them, to present, on his part, a male and female Torpedo in spirits; in the latter of which the electric organs are exposed in different views and sections; likewise a copper-plate, which he took care to have engraved, exhibiting those organs.

Of the general structure and anatomy of the Torpedo I say nothing, since the animal does not differ very materially, excepting in it's electric organs (as they have been properly named by Mr. Walsh) from the rest of the Rays, of which family it is well known to be. I will only premise, that the Torpedo, of which I treat, is about eighteen inches long, twelve broad, and in it's central or thickest part two inches thick; which is nearly the size of

the female specimen, now presented to the Society, as well as of that from which the plate was taken: but where there is any difference in the organ arising from difference in size, notice will be taken of it in this account.

The electric organs of the *Torpedo* are placed on each side of the *cranium* and gills, reaching from thence to the semicircular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal to the transverse cartilage, which divides the thorax from the abdomen; and within these limits they occupy the whole space between the skin of the upper and of the under surfaces: they are thickest at the edges 'near the center of the fish, and become gradually thinner towards the extremities. Each electric organ, at it's inner longitudinal edge, is unequally hollowed; being exactly fitted to the irregular projections of the *cranium* and gills. The outer longitudinal edge is a convex elliptic curve. The anterior extremity of each organ, makes the section of a small circle; and the posterior extremity makes nearly a right angle with the inner edge. Each organ is attached to the surrounding parts by a close cellular membrane, and also by short and strong tendinous fibres, which pass directly across, from it's outer edge, to the semicircular cartilages.

They are covered, above and below, by the common skin of the animal; under which there is a thin *fascia* spread over the whole organ. This is composed of fibres, which run longitudinally, or in the direction of the body of the animal: these fibres appear to be perforated in innumerable places; which

gives the *fascia* the appearance of being fasciculated ; its edges all around, are closely connected to the skin, and at last appear to be lost, or to degenerate into the common cellular membrane of the skin.

Immediately under this, is another membrane, exactly of the same kind, the fibres of which in some measure decussate those of the former, passing from the middle line of the body outwards and backwards. The inner edge of this, is lost with the first described ; the anterior, outer, and posterior edges are partly attached to the semi-circular cartilages, and partly lost in the common cellular membrane.

This inner *fascia* appears to be continued into the electric organ, by so many processes, and thereby makes the membranous sides or sheaths of the columns, which are presently to be described ; and between these processes the *fascia* covers the end of each column, making the outermost or first partition.

Each organ, of the fish under consideration, is about five inches in length, and at the anterior end three in breadth, though it is but little more than half as broad at the posterior extremity.

Each consists wholly of perpendicular columns, reaching from the upper to the under surface of the body, and varying in their lengths, according to the thickness of the parts of the body where they are placed ; the longest column being about an inch and an half, the shortest about one fourth of an inch in length, and their diameters about two tenths of an inch.

The figures of the columns are very irregular, varying according to situation and other circumstances. The greatest number of them are either irregular Hexagons, or irregular Pentagons; but from the irregularity of some of them, it happens that a pretty regular quadrangular column is sometimes formed. Those of the exterior row are either quadrangular or hexagonal; having one side external, two lateral, and either one or two internal. In the second row they are mostly pentagons.

Their coats are very thin, and seem transparent, closely connected with each other, having a kind of loose network of tendinous fibres, passing transversely and obliquely between the columns, and uniting them more firmly together. These are mostly observable where the large trunks of the nerves pass. The columns are also attached by strong inelastic fibres, passing directly from the one to the other.

The number of columns in different Torpedos of the size of that now offered to the Society, appeared to be about 470 in each organ, but the number varies according to the size of the fish*. These columns increase, not only in size, but in number, during the growth of the animal: new ones forming perhaps every year on the exterior edges, as there they are much the smallest. This process may be similar to the formation of new teeth, in the human jaw, as it increases.

* In a very large Torpedo, the number of columns in one electric organ were 1182.

Each column is divided by horizontal partitions, placed over each other, at very small distances, and forming numerous interstices, which appear to contain a fluid. These partitions consist of a very thin membrane, considerably transparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inside of the columns. They are not totally detached from one another: I have found them adhering, at different places, by blood-vessels passing from one to another.

The number of partitions contained in a column of one inch in length, of a Torpedo which had been preserved in proof spirit, appeared upon a careful examination to be one hundred and fifty: and this number in a given length of column appears to be common to all sizes in the same state of humidity, for by drying they may be greatly altered; whence it appears probable that the increase in the length of a column, during the growth of the animal, does not enlarge the distance between each partition in proportion to that growth; but that new partitions are formed, and added to the extremity of the column from the *fascia*.

The partitions are very vascular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of respiration. They pass along with the nerves to the electric organ, and enter with them; then they ramify, in every direction, into innumerable small branches upon the sides of the columns, sending in from the circumference all around upon each partition small arteries,

arteries, which ramify and anastomose upon it; and passing also from one partition to another, anastomose with the vessels of the adjacent partitions.

The veins of the electric organ pass out, close to the nerves, and run between the gills, to the auricle of the heart.

The nerves inserted into each electric organ, arise by three very large trunks from the lateral and posterior part of the brain. The first of these, in its passage outwards, turns round a cartilage of the *cranium*, and sends a few branches to the first gill, and to the anterior part of the head, and then passes into the organ towards its anterior extremity. The second trunk enters the gills between the first and second openings, and, after furnishing it with small branches, passes into the organ near its middle. The third trunk, after leaving the skull, divides itself into two branches, which pass to the electric organ through the gills; one between the second and third openings, the other between the third and fourth, giving small branches to the gill itself. These nerves having entered the organs, ramify in every direction, between the columns, and send in small branches upon each partition where they are lost.

The magnitude and the number of the nerves bestowed on these organs, in proportion to their size, must on reflection appear as extraordinary as the phenomena they afford. Nerves are given to parts either for sensation or action. Now if we except the more important senses of seeing, hearing, smelling, and tasting, which do not belong to the electric organs, there is no part even of the most perfect animal, which, in proportion to its size, is
to

so liberally supplied with nerves; nor do the nerves seem necessary for any sensation which can be supposed to belong to the electric organs. And with respect to action, there is no part of any animal, with which I am acquainted, however strong and constant it's natural actions may be, which has so great a proportion of nerves.

If it be then probable, that those nerves are not necessary for the purposes of sensation, or action, may we not conclude that they are subservient to the formation, collection, or management of the electric fluid; especially as it appears evident, from Mr. Walfh's experiments, that the will of the animal does absolutely controul the electric powers of it's body; which must depend on the energy of the nerves.

How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations, time and future discoveries alone can fully determine.

AN EXPLANATION of the Engraving of the
T O R P E D O.

TAB. XX.

Fig. I. The upper surface of the electric organ.

AA. The common skin of the animal.

B. The inspiratory opening.

C. The eye.

D. The part in which the gills are inclosed.

EEE. The skin dissected off from the electric organ, and turned outwards; the honeycomb appearance on it's internal surface corresponding with the upper surface of the organ.

F. The part of the skin which covered the gills, with some ramifications of an excretory duct upon it.

GGG. The upper surface of the electric organ, formed by the upper extremities of the perpendicular columns.

Fig. II. The right electric organ divided horizontally into nearly two equal parts at the place where the nerves enter; the upper half being turned outwards.

AA. BB. CC. DD. The corresponding parts of the trunks of the nerves, as they emerge from the gills, and ramify in the electric organ.

AA. The first or anterior trunk arising just before the gills.

BB. The second or middle trunk arising behind the first gill.

E. The

- cc. The anterior branch of the third trunk arising behind the second gill.
- DD. The posterior branch of the third trunk arising behind the third gill.

Fig. III. A perpendicular section of the Torpedo a little below its inspiratory openings.

- AA. The upper surface of the fish.
- BB. The muscles of the back as divided by the section.
- c. The Medulla Spinalis.
- D. The Oesophagus.
- E. The left gill split, to expose the course of a trunk of the nerve through it.
- F. The breathing surface of the right gill.
- GG. The fins.
- HH. The perpendicular columns which compose the electric organ, with a representation of their horizontal partitions.
- I. One of the trunks of the nerves, with its ramifications.

Fig.

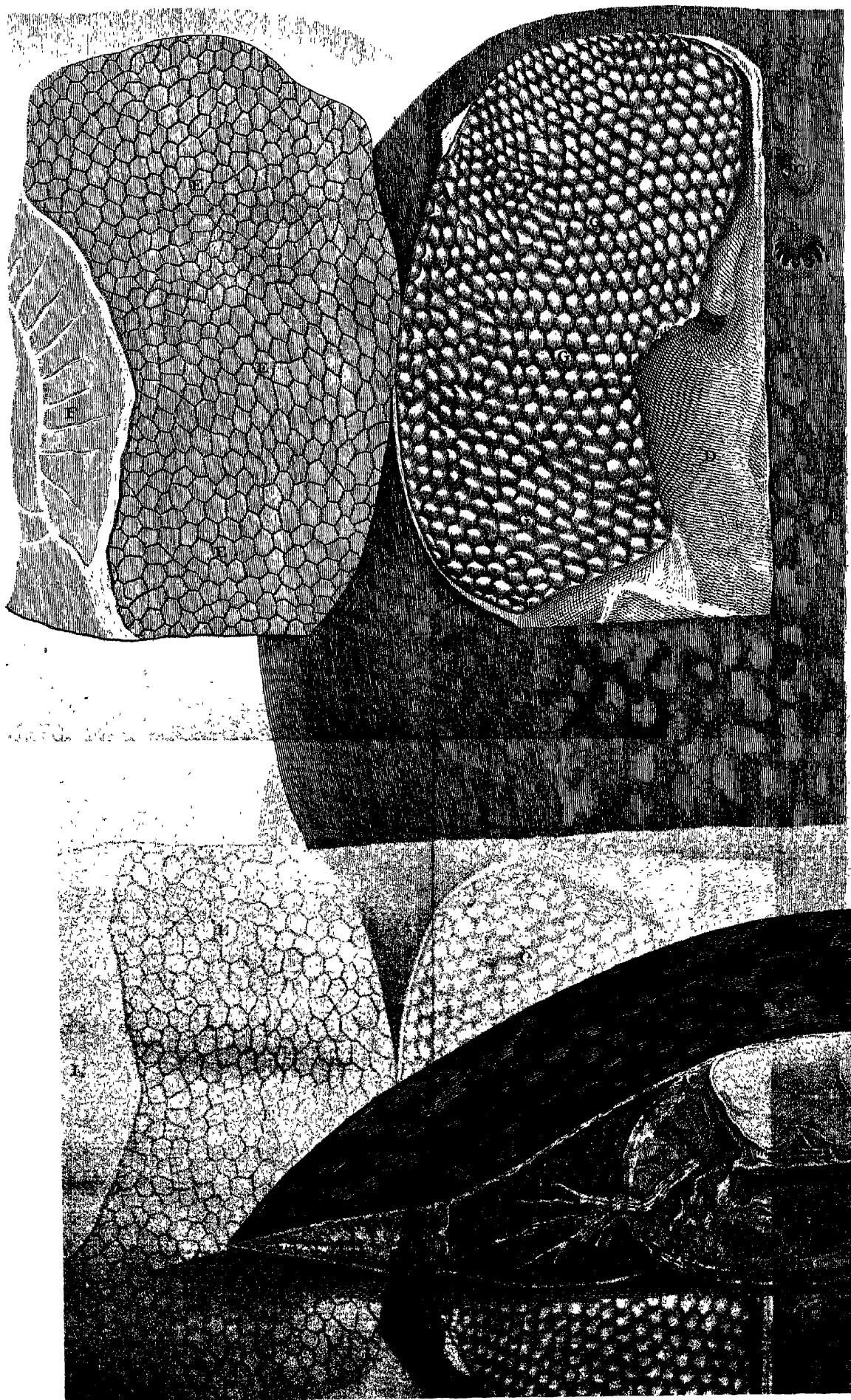


Fig. 2

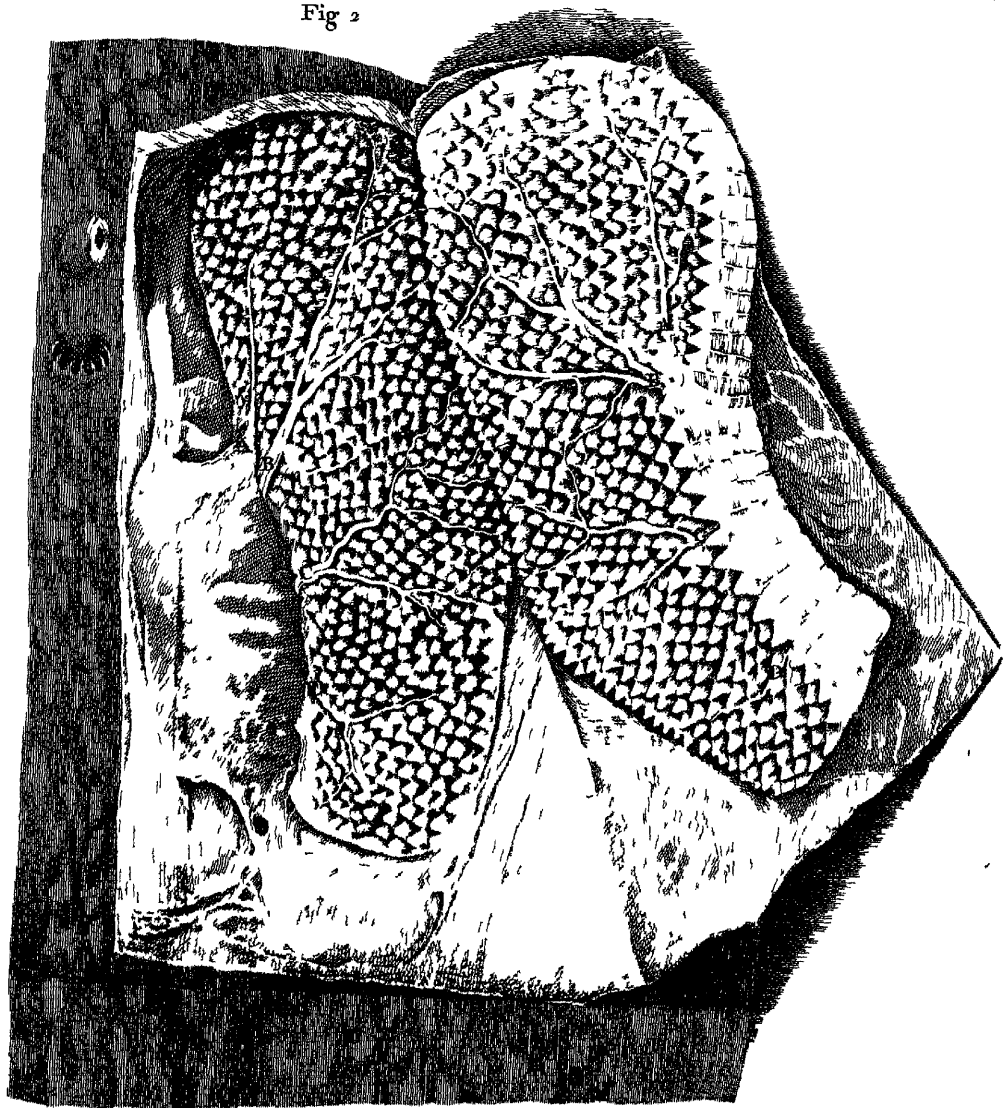
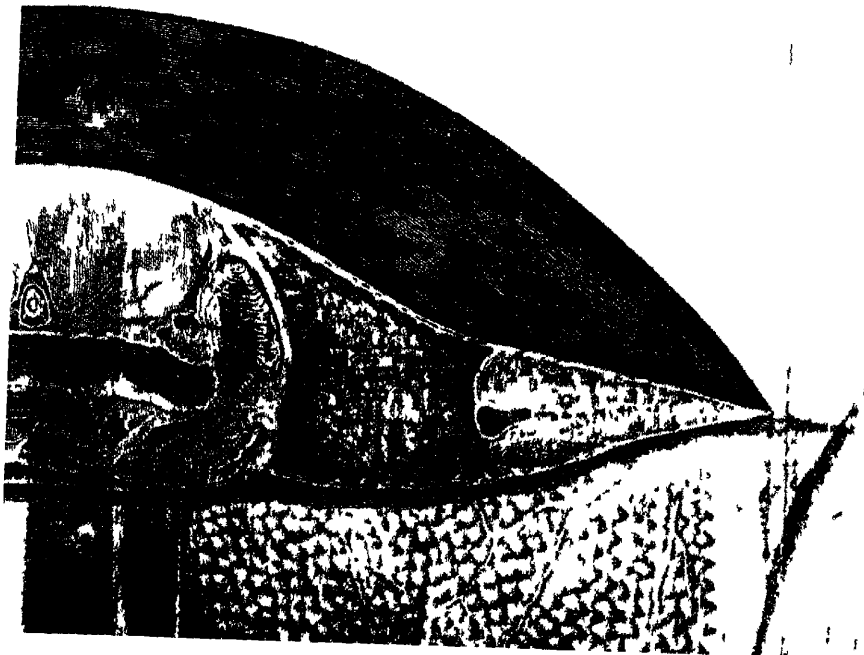


Fig. 3



Dr. Robert de la Roche sculp.

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